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Editorial Notes

THE Notes in our last number were written under holiday conditions. The position is now reversed, and these will be read (if at all) by readers on holiday or fresh from it. That—and the usual pressure of space—is why they are so short. We shall revert to the usual length of three pages in December. The pressure is greatest in the review section, and in order to relieve it we are publishing far more reviews than usual in this number. We should welcome comment from readers as a guide to the future. The extra space is obtained at the expense of articles and notes; is this change approved or not? We do not contemplate publishing quite so many reviews as this in every number; but if the review section continues to be popular we might well give more space to it. (We cannot undertake to reply to any letters we may receive but thank the writers now in anticipation).

The first absolute date from the radiocarbon method has been obtained from wood found in the mesolithic settlement at Star Carr, excavated by Dr Grahame Clark. The date is still confidential, but we may say that it agrees quite nicely with dates already in vogue for the Boreal period. The preliminary report on the 1949 excavations is published in the current number of the *Proceedings of the Prehistoric Society* (New Series, vol. 15; Hon. Secretary, Mr T. G. E. Powell, Dept. of Prehistoric Archaeology, The University, Liverpool 3), of which Dr Clark is the Editor. He concludes that Star Carr is 'one of the richest and most informative sites of the Maglemosian culture anywhere in north-western Europe', and that the site belongs to 'an early stage [in its history], one in which upper palaeolithic traditions are still perceptible in the choice of antler rather than bone, and in the methods used for working it'; and he cites in comparison the technique in use at the famous Late Glacial site near Hamburg. We are thus rapidly discovering exactly how the cultures of the Old Stone Age evolved into those of the New, during the intervening mesolithic period; and it would be very helpful if specimens of wood from the Hamburg site could be subjected to the radiocarbon process. (The procedure to be adopted was described by Dr Hallam Movius in our last number, pp. 99-101). The excavations at Star Carr are being continued this year and are in progress as we go to press.

We regret that, in mentioning Dr Grahame Clark's 'Report on Excavations on the Cambridgeshire Car Dyke, 1947' in our last number, under the heading of 'Important New Books and Articles', we carelessly omitted to say where it was published. It appeared in the *Antiquaries Journal* for July-October, 1949 (Vol. xxix, pp. 145-163).

Maps and the Medieval Landscape

by M. W. BERESFORD

BY confining themselves to a small-scale map and broad, easily generalized data, the Ordnance Survey have pioneered in the mapping of historical landscapes. When it comes to the medieval landscape their technique is less happy: for what economic historians (and, I suspect historical geographers) need is not more generalized detail—that they have already in verbal sources. What they look for is more precise detail. The questions which still remain unanswered in medieval economic history are essentially local questions, for which large-scale mapping of local topography would be needed, if the answer were to be presented in map form.

To take one theme as example: the colonization of forest England by the medieval villager. How far did the open-field colonist penetrate? How much was the physical form of the open-field village modified in marginal geographical areas? Was the individual settler in his forest-clearing an independent man from the very beginning of the strip method of cultivation, which formed the prop as well as the prison of the nucleated village economy? Did soil types have a strong influence on the speed and direction of the expansion of the village fields outward, and on the slow shaving of the waste? Did soils affect the propensity to enclose? Is the present pattern of settlement a high water mark concealing earlier ebbs and flows, or was medieval England more densely colonized than the England of Arthur Young?

If these questions are to be answered the historian will need good maps. If these do not exist he will have to make them. The research necessary to answer such questions as these will be partly in documents and partly on the ground itself.

To expect help from medieval map-makers will be in vain. The few medieval maps are both small-scale and pictorial. The large-scale map or plan is extremely rare before the mid-sixteenth century (1). From that date the craft of surveyor achieved precision and full employment in the service of the many Tudor landlords who were buying or selling land, or in the course of the litigation which itself (from the profusion of documents it evoked) illuminates that litigious age for us.

The large-scale plans of the Elizabethan surveyors exist in sufficient number to whet our appetite for more. In general, they are both accurate and informative and they present us with the open-field village well before its physical dissolution at enclosure. Such maps as those at All Souls', Oxford (2), depict a landscape which would have been familiar in its physical aspect to the founder of the College, even if the distribution of names among the strips is not quite in the medieval vein. Other Elizabethan maps catch the village in the act of dissolution—half in open-field and half in newly-hedged enclosures, the hedges in the fields as new as the bright green trees and hedges of the cartographer. One of the All Souls' maps marks the site of a village which had been abandoned during the sheep enclosures of the early 16th century, and it faithfully records the extinguished pattern of the strips upon which the villagers once worked (3).

¹ One of these rare examples (Chertsey Abbey, 1432) is printed in Edward Lynam, *British Maps and Map-makers* (London 1944), p. 16. Curiously enough there is no open field map reproduced in this book. Part of a Tudor village plan will be found at page 21.

² These have been transcribed by R. H. Tawney in part in his *Agrarian Problem in the Sixteenth century*. Comparison of these transcripts with the originals reveals many errors of transcription. The best of the All Souls' maps is that of Padbury, Bucks, where almost every acre of the parish is covered with strips.

³ The half-way map is that of [Weedon] Weston, Northants. The lost village is shown on the Whatborough map: 'the place where the town of Whatborough stood'.

These 16th century maps, few as they are, coupled with the estate plans of the next two centuries give for most English counties a sample of open field landscapes. Lucky counties like Bedford may count their open field maps in scores; less lucky in ones or twos (4). Doubtless more maps still lie in their estate office dust, their value unperceived. But even with the emergence of these into daylight, the open field plans will be no more than a sample. Economic historians are a little nervous of samples, as they see the revisions which have had to be made in the last decade of generalizations based on samples. And samples, from their very nature, will not serve to answer the essentially local questions which have been asked above.

Fortunately the close inspection of open field maps provides a hint of how we may make our own open-field maps for our own local enquiries. The essence of an open-field map is that it should show the position of the single strips of the arable area; the location of land which did not form part of the arable, but was either pasture, meadow, wood or heath; the number and location of the big field units by which the rotation of fallow moved; and the roads and balks which led the peasant to the strip or to the wood or to the town. It is clear that such a map demands something like a $2\frac{1}{2}$ inch scale, if it is to be adequate in its delineation of detail.

In many parts of England such a reconstruction is possible. It hinges on an equation whose truth I have argued elsewhere (5), and which here I must ask to be taken on trust. When I put this equation to people there are usually two responses: some say—as I hope do the readers of *ANTIQUITY*—‘Oh, I always knew that’; and others say, ‘Oh, nonsense’. The controversial equation is the simple one that the single strip of the medieval fields is represented exactly by the ridge and furrow of the modern English landscape. From one green furrow, up and over the curved ridge, and down to the next furrow: this twenty-two feet or so is the *selion*, the *land*, the *ridge* of all open-field documents from the 12th century to the 19th. The length of the strip, very roughly approximating to the furrow-length (or fur-long) of 220 yards is the length of the ridge-and-furrow.

There are two simple ways of proving this equation before using it. The first is to take a pre-enclosure strip-map and compare it with the ridge and furrow. It will be found that the strip on the map is represented by the ridge on the ground. Where no such maps survive, a quick inspection of a few fields of ridged land will soon show some point where the ridging passes under a hedge, through a road, or over a canal in a practical demonstration of its priority over these post-enclosure, man-made obstacles to its pattern.

With the equation we may move out into the fields armed with a $2\frac{1}{2}$ inch map. Before long the map will be covered with lines marking the direction of the strips, the boundaries where the strips change direction, and figures showing the number of strips

⁴ The Beds. maps are at the County Record Office. The Essex Record Office has published *The Art of the Map Maker in Essex 1566-1860* (Chelmsford, 1947). There is no complete printed list of extant open-field maps. In the pre-war *List of Cadastral Maps* the Ordnance Survey indicated those which it had photographed. Negatives and prints were bombed, but prints of many are in the British Museum map-room. In 1888 R. B. Mowat printed *Sixteen Old Maps of Oxfordshire*.

⁵ ‘Ridge and Furrow and the Open Fields’ in *Economic History Review* (new series) 1, 34 (1948). Open field maps are here printed side by side with air photographs of the same area showing ridge and furrow. See also Plates 28-9 of *Wessex from the Air* where Mr O. G. S. Crawford made a similar comparison for Calstone, Wilts.

within each of these blocks—the *furlong* or *cultura* of the medieval field. It will be found easier to outline the bundles of strips and represent their number by a figure, than to draw in each individual strip. If each strip is to be drawn, then a 6 inch or a 25 inch sheet will be needed.

It will be very likely that a number of blanks will be drawn. Some modern fields will bear no sign of ridging at all. If such fields are near a stream or on the far borders of the parish they may well represent land that was never part of the communal arable. If they are nearer the village then their absence may be due to zealous ploughing out since enclosure. In such cases the aid of air photographs may be invoked. From the air the soil discolorations and the crop-marks which result from the old ridging of the ground will be much clearer, and some blanks on the map can be filled.

In those parts of England which have light soils, easily ploughed, the erasure of the pre-enclosure pattern will be most complete. In southern, central and northern England I have found the pattern remarkably persistent and few railway journeys can be undertaken without seeing it.

In cases where no ridging is to be seen either from the ground or from the air it would be rash to argue the absence of open fields without the support of documents. But experiments in marginal areas such as the Pennine slopes, the Yorkshire Wolds, the Wirral, Shropshire and the Peak District have shown that in these areas the erasure of ridge and furrow is slight; and it is precisely in such areas that geographical factors begin to militate against the open field form of agriculture, and where we may see the open field economy adjusting itself to the strict discipline of a narrow Pennine Dale or a steep hilly slope of the Cotswolds.

For naming the *furlongs* the eye must turn from the fields and the air photograph to the documentary sources. These are the minor field-names which occur in almost every medieval charter or land-document. It is in these furlongs that the strip is identified as lying 'between the *selion* of John the Miller to the eastwards and William atte Green to the west, the highway being at the north and the stream called Hallebreck to the south'. One way of collecting such names would be to search every medieval document covering a given area. Such advice is a counsel of more than perfection. But there is a source which helps us to extract the more important names of the two, three or four open fields of the village in which we are interested, as well as a small number of the minor furlong names. These documents, virtually unused as yet by historians, are the glebe terriers: the written surveys in which the territory forming the church lands were recorded and sent to the Bishop. Most dioceses have a bulky collection of such documents: if you are lucky the earliest will date from the 1570's; and in most cases the series will be certain to begin about the middle of the next century⁽⁶⁾.

In such documents the land described will, in most open-field villages, be exactly like other land there. It will consist of many scattered strips, and in order to describe them accurately the terrier will have to name the Field in which the strip lies, the furlong where it is to be found, and its position in the furlong—either whose strip is lying next to it on either side, or its position in relation to the bounds of the furlong—'being ye seventh and ye fifteenth lands from ye northwards'. In very many terriers which I have examined in the Lichfield, Coventry, Worcester, Lincoln and York dioceses the eye is aided by a tidy habit on the part of the 17th century clergy: the parchment is divided into columns, one for each Field, and the field name is written boldly at the head

⁶ I have described and transcribed the Leicestershire terriers in W. G. Hoskins (ed.) *Studies in Leics. Agrarian History* (Leicester, 1949), pp. 77-126. The Yorkshire terriers are dealt with in *Yorks. Archaeological Journal*, 1950.

of the column. Within each column a line divides furlong from furlong, with the furlong name indented in the left margin. With such a document the essential names can be extracted in a matter of seconds.

We now have some names on our map, and in fact rather more field names and detail than a modern 6-inch map will give us for our modern landscape. The plotting of the strips will probably have revealed the fossilized road-ways of the open field village, the *balks* running between the furlongs. Some of these will have been converted into modern lanes, whose sudden right-angled turn reflects the arrival of the furlong-end. Others will be (significantly) footpaths on the Ordnance Map. Others will be broad grass elevations threading their way among the fields, in step with the ridges and furrows which they once served, but out of step with the modern hedges and ditches which have been laid across them.

Our reconstruction has so far ignored the village itself. In the nucleated village the houses were grouped at the core of the fields. The farm-house out in the fields is a modern creation, its enclosure-born date often signified in its historical name—Waterloo Farm, Quebec Farm, Wellington Farm.

When we come to the village we are badly hampered by later development. The 17th century saw the arrival in the village of a firmly-built brick, wooden or stone cottage which has overlaid the medieval cot. Many villages have expanded along the street but it is impossible to say from looking at houses exactly how far the medieval village ran. We shall have the site of the medieval church, and if the restorers have not been at work we shall have something of its structure. We may have the medieval manor house, and if we have not the mill itself we shall probably have its site in a field-name or in a mound and ditch.

To obtain a precise picture of a medieval village laid out along its streets we should be able to call in the archaeologist. But the traditional interests of English archaeology have not laid in the direction of medieval villages, and it must be said in their defence that modern villagers have never taken too kindly to the suggestion that their house-floors should be removed and the garden pitted in the pursuit of knowledge.

The gap is within sight of being filled by recourse to sites of villages which have been abandoned. It is never quite as useful to dissect a corpse as it is to dissect a living body, but the very fact that we have a corpse on our hands provides us with a fresh set of historical questions to be answered: why did the village die? Indeed the profusion of lost village sites which can be traced once a thorough search is begun, makes it necessary to pose another question: why did so many sites find themselves abandoned? Or in another form, that may be put: how many villages *were* there altogether in (say) 1250?

Such abandoned sites have preserved for us the village streets: these are now deep depressions through the grass, their right angles a little smoothed by the grass which grows over them and the soil which has been blown and washed into them. But it is not necessary to go into the air to see them. Once recognized, they are very clear to the ground observer. The buildings of abandoned villages have rarely remained above ground. We found the floor of the church at Stretton Baskerville (Warws.) a foot below the turf, with the broken floor tiles and the roof slates which had fallen on them (7). For the church walls we had to go to a nearby barn, where some of the worked stone had been used; some had been used to dam the outlet of the fine set of medieval fish-ponds; and some were in use at the manor House some half mile or so away as the sides of a sunken tennis court. In other villages, such as Cestersover (Warws.) the church has been used

⁷ I am describing this and other Warwickshire sites in the Birmingham and Midland Archaeological Society's *Transactions*, vol. 66 (in the press).

as a barn ; in others, like Wharram Percy (East Riding) it survives as the only building in the grassy fields where its parishioners once lived (8).

With building stone valuable, it is not surprising that any worked stone from lesser buildings has been carried away to other villages. What trial excavations have revealed is very little stonework other than rubble foundations or stone hearth-bases. No large buildings, apart from churches, have been excavated, although at Wharram Percy a particularly clear air photograph is an excavator's blue-print of a many-roomed house. What remains of cottages above ground is little more than a hollow depression or the raised platform on which a house stood. The boundary mounds of cottage, toft and croft, field and garden are rather more clear, and in many of them the right-angled turns are virtually unobscured by erosion.

It is to such lonely fields that we must go if we are to see the unimpaired ground-plan of the medieval village. In most counties the journey need not be all that far. The abandoned villages of Lincolnshire were listed by Canon Foster as long ago as 1924, and one of them was pictured in an air photograph by O. G. S. Crawford. A site near Oxford was excavated before the War, and in 1945 Dr W. G. Hoskins published his list and analysis of the Leicestershire villages. His map locates 49 sites and others have been added since (9). In Warwickshire I have completed a similar survey, and about sixty have been mapped. Similar surveys of the three Yorkshire Ridings are well on the way, with indications that the joint total may exceed a hundred villages and hamlets. A rough glance at Buckinghamshire, Northamptonshire and Oxfordshire indicate that there is abundant material there also ; and sites in Derbyshire, Durham, Nottinghamshire and Cambridgeshire have been noted.

In the production of such maps as I have described, the selfish need of economic history research has been the main goad. (10) The method has been described here in the hope that it may be applied by others in other parts of the country for this and for other purposes. For enquiries where large-scale detail of the medieval landscape is needed I believe that this approach, combining field-work, air photographs and documentary evidence will provide something of a consolation where open field maps are missing. A 2½ inch map with such detail added will be able to answer some of the questions which we would put to a Tudor survey : but it is unlikely—at least in my hands—to have the pictorial qualities which make such maps as those at All Souls' or at Chelmsford such a delight to handle.

⁸ The air photograph references of these two sites are : Wharram Percy, CPE UK 2593/3030 ; Cestersover, CPE UK 1925/2005 and 4003. I have given short accounts, with photographs of both the Ridge and Furrow and the Lost village themes in two articles in 'Country Life'—'What is Ridge and Furrow' (4 March 1949) (Plates of 1593 map of Weston, Northants ; air photographs of that area and of East Newton, E.R. Yorks. ; ground photographs of Ilmington, Warws. ; map of 1737 of Allestree, Derbys.). 'Tracing Lost Villages' *ibid.*, 15 October 1948). (Plates of 1586 map of Whatborough, Leics., with air photograph of the site ; air photographs of Cottam, E.R. Yorks. ; Upper Radbourn, Warws. ; Cestersover, Warws.). Two other air photographs of field-patterns will be found in the article cited above in the *Yorks. Arch. Journal*, 1950 (of Wighill, W.R., and of Hornby, N.R.).

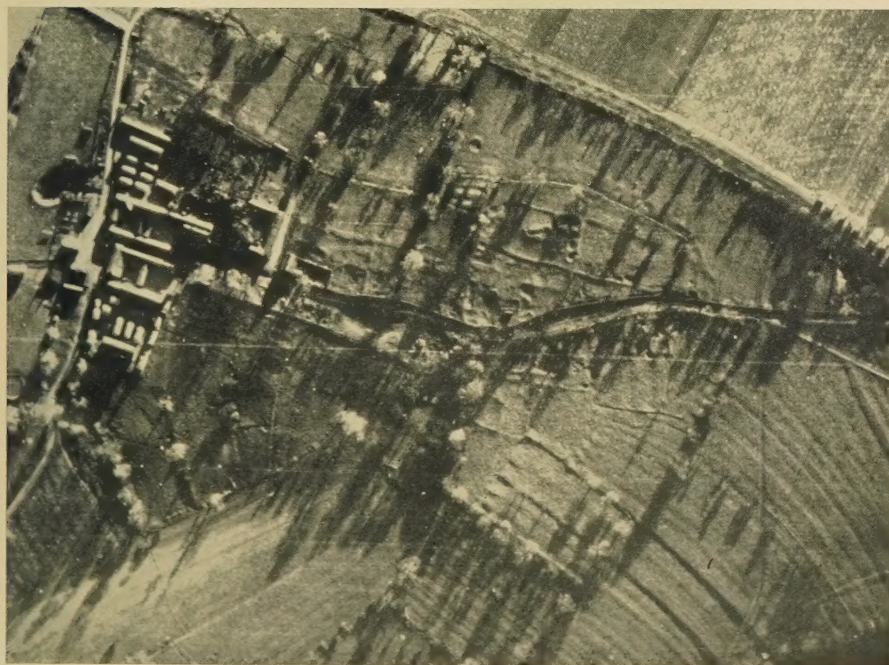
⁹ Lincoln Record Society, volume 19 (1924) *The Lincolnshire Domesday*. O. G. S. Crawford in *Antiquaries Journal*, 1925, p. 432, for photograph of Gaisthorpe. The Berks. site is described in *Oxoniensis* v, p. 31 (1940) by Bruce Mitford. Dr Hoskins' article is in *Trans. Leics. Archaeological Society*, xxii, 242.

¹⁰ Smaller scale maps could be used for the recording of manorial boundaries, distribution of monastic property, manor houses, castles, mills, etc. I believe that Colonel Drew is compiling such a tenurial topography.

PLATE I



THE DESERTED SITE OF COTTAM (E.R. YORKS) SHOWING THE
STREETS BEHIND THE HOUSES, THE FIRST
FURLONG OF THE OPEN FIELDS
(O.S. Grid ref. 44/992648)



THE DESERTED SITE OF TOWTHORPE (E.R. YORKS) WITH THE STREET,
HOUSES AND CLOSES, AND THE MODERN FARM OVER-LYING PART OF
THE SITE. THE MEDIEVAL PLOUGHLANDS SWEEP UP TO THE SITE
(O.S. Grid, ref. 44/900629)

PLATE II



HUTS ON THE PALATINE HILL, ROME, FOUND 1948

Huts on the Palatine Hill, Rome

by S. M. PUGLISI

THE discovery of primitive hut-foundations within the walls of Rome is an event of some importance, throwing light on the prehistoric origins of that great city.

These hut-foundations were found during excavations carried out under the general direction of Professor P. Romanelli in the area of Germalus, and my own share in the work was concerned with the pre- and protohistoric remains found (1).

Earlier observations by Vaglieri on the Germalus (2) and by Boni under the Domus Flavia (3) had, in fact, proved the existence of a village of the Early Iron Age (1st and 2nd phases of Latian culture) extending over both the Palatine hill-tops. The main objects of the present excavation were to find an area undisturbed by the intensive building of republican and imperial times, to make a stratigraphical survey, and to re-examine the surface remains interpreted by Vaglieri as those of a cemetery (4). The heated controversy between Vaglieri and Pigorini about the nature of the finds did not settle the problem, but merely brought the excavations to a standstill. Vaglieri's contention was refuted both by a re-examination of the archaeological material in the Antiquario Palatino and by the remains found by Boni under the Domus Flavia. Both suggested domestic activities—the remains of baked clay ovens and potsherds, wattle and daub fragments of hut walls (Boni excavations), and food refuse such as broken and burnt animal bones.

The surface on which the huts under the Domus Flavia were built was the upper layer of the fluvio-lacustrine deposits which rest on the tufa rock (5). Although Boni's photographs are difficult to interpret, they do show some post-holes. But the uncompact nature of the deposits evidently made observation more difficult than on the Germalus site where, on the lower crest of the hill, the upper deposits had been removed by the action of rain, exposing the hard tufa surface on which the primitive inhabitants could build their huts (6). Channels dug round the huts (PLATE II) were evidently designed as a protection against rain-water. A similar device was observed by Mengarelli at Satricum (7) and by Rellini at Punta Manaccore (8). Remains of two such dwellings and parts of a third were found by Vaglieri. On the Germalus site the deposits fortunately were still undisturbed, owing to the building (during the middle republican period) of a wall of large yellow tufa blocks. Within this large hut, whose structure was later found to be almost completely preserved, excavation was carried out with strict attention to stratigraphical

¹ See my Report read at a meeting of the Istituto di Antropologia, 29 May 1948.

² *Not. Scav.*, 1907, 185, 264, 444, 529.

³ Report partially published by M. Marella Vianello in *Antichità*, I, 1947.

⁴ See *RAL.*, xvi, 2; xvii, 1; xviii, 5.

⁵ Section published by De Angelis d'Ossat in *Per la ricerca del Lupercalc, Bull. Comm. Arch. Com. di Roma*, LXII, 1934, 79.

⁶ The geological aspect of the recent excavations has been dealt with by G. De Angelis d'Ossat.

⁷ G. Pinza, *Mon. Ant. Lincei*, xv, 1905, 480 (*Mon. primitivi di Roma e del Lazio antico*).

⁸ *BPI*, LIV, 1934, 12-13.

method. The results provide the first objective evidence for the succession of cultures on the hill from its first occupation down to the 3rd century B.C. Of the four strata observed in this area the first two belong to the 1st and 2nd phases of the Latian Iron Age, which are already well known from burials in the Alban and Roman hills and particularly from Boni's famous excavation in the Forum cemetery (9). The following description deals only with the stratigraphy of the earliest archaic period.

Stratum 1 is a thin layer of clayey sand with organic remains, immediately overlying the hut floor. It yielded wattle and daub fragments, charcoal and ash marking the site of a central hearth, small fragments (probably broken up by walking over them) of burnished and unburnished hand-made pots with incised geometric ornament. There were also remains of good debris. The probable date is about 800-700 B.C.

Stratum 2 is thicker and was deposited after the hut was disused. The soil has a greater organic content (refuse dump?). Most of the pottery is still not wheel-turned, but the upper ring of the double handle is more developed, and twisted handles and red-burnished ware appear. Decoration consists of finely incised straight or curvilinear designs including semicircles and double spirals. Some *bucchero* ware occurs, together with local imitations of early Greek forms (oinochoai, skyphoi, kilikes). Significant are fragments of proto-geometric painted pottery undoubtedly imported from Hellenic or hellenized centres (proto-Corinthic style), and fragments of the Italo-geometric ware equivalent thereto. This stratum is quite homogeneous and full of well defined fragments, and it corresponds to the 2nd phase of the Latian cemeteries, typified by Riserva del Truglio (10). The probable date is about 700-550 B.C.

Strata 3 and 4 contain objects of the republican age; below them is a transitional level with some remains of the 2nd phase.

The attribution of this occupation of Germalus and Palatium to hut-dwellers of the earliest Iron Age is based upon the following facts:—(1) the agreement of the material from the first two strata with that from the Forum cemetery which is clearly to be associated with this village: (2) the presence in the large hut of these first two strata: (3) The structure of the dwelling found during the recent excavations. This recently revealed hut is of sub-rectangular shape and measures about 3.65 by 4.80 metres; it is dug out of the tufa which here is marked by *pozzuolana*. The traditional half-sunk Italian hut typical from neolithic and eneolithic times (11) is here still in use; the depth of its sunken area ranges from 0.50 to 0.90 metres. The entrance faces south and has a kind of step. Round the edge of the hut are post-holes dug to hold up the roof-supports.

A large hole found in the centre of the hut solves once and for all the problem of the structure of Latian dwellings, imaginatively reconstructed on the basis of the pottery hut-urns (12). Confidence in such reconstructions was weakened by the conventional character of the pottery models which also naturally, being meant to hold ashes, gave no clue to the internal arrangements of the originals. Now, as a result of collaboration with Mr A. Davico, an architect, the general similarity in plan and structure between the Germalus hut and those represented by the Early Iron Age Latian hut-urns has been fully established. Outside the Germalus hut on either side of the entrance are two holes for posts supporting a projection of the sloping roof, or perhaps for a small porch such

⁹ *Not. Scav.*, 1902 (3); 1903 (4, 8); 1905 (6); 1906 (1, 7); 1911(3).

¹⁰ U. Antonielli, *BPI.*, XLIV, 1924.

¹¹ Cf. G. A. Colini in *BPI.*, xxxii, 1907 (Scoperte arch. nella valle della Vibrata); U. Rellini in *Not. Scav.*, 1925 (Scavi preistorici a Serra d'Alto).

¹² *Not. Scav.*, 1893, 204 ff.

HUTS ON THE PALATINE HILL, ROME

as that shown in an urn from Campo Fattore now in the Prehistoric Museum, Rome. A cutting on the right hand side (looking outwards from the hut towards the entrance) corresponds exactly to a window seen in the same position in many Latian and South Etrurian hut-urns. The stability of the turtle-like roof as is shown by the hut-urns was secured by means of a central pillar of considerable size, as may be seen from the diameter (0.45 m.) and depth (0.55 m.) of the hole. The hut-urns show a horizontal beam in the roof much shorter than the diameter of the hut; this was evidently designed to support the weight of the roof. The remains of the hearth were fairly close to the central pillar, but the risk of fire was avoided by the use of a closed flame oven such as was in use already in the Bronze Age by the Apennine people (13).

We may conclude that people with a uniform culture were settled in various places in Latium, including the Roman hill, during the first part of the Iron Age; and it is not necessary to accept the traditional priority of settlements in the Alban Hills (14). On this point the evidence from cremation and inhumation burials in the Forum agrees with that from our village. Physical anthropology can tell us nothing about the cremating peoples, but it recognizes the Mediterranean characteristics of the Latian inhabitants who practised inhumation (15). Moreover the practice of cremation—widespread in Europe and appearing first in the Po valley during the Bronze Age—probably spread rather by cultural diffusion than by ethnical migration.

The spread of the hut-urns throughout west central Italy, and their points of difference from those made elsewhere (16), and above all their presence in regions of mixed burial ritual, suggest that the adoption of cremation was general and not necessarily connected with a particular culture, much less with a particular ethnic group. We may recall the use of huts as burial-places by the Mediterranean peoples of Italy (17); the use of hut-urns might well be a sort of graft on this practice, just as megalithic burial-chambers might be regarded as 'houses of the dead' (18).

ABBREVIATIONS

RAL = Rendiconti dell' Accademia dei Lincei (classe scienze morali).

Not. Scav. = Notizie degli Scavi.

BPI = Bullettino di Paletnologia Italiana.

¹³ Cf. U. Rellini, *Not. Scav.*, 1931, 184 (Nuove ricerche al pianello di Genga e nella Gola del Sentino).

¹⁴ P. Ducati, *Come nacque Roma* (Rome, 1939), p. 144.

¹⁵ On this see G. Sergi, *Italia: le origini*, ed. Bocca, 1919; S. Sergi, *Antropologia laziale*, published in 'Le scienze fisiche e biologiche in Roma e nel Lazio', *Ist. Studi Romani*, 1933; G. Sergi, *Da Alba Longa a Roma*, Turin, 1934.

¹⁶ See Behn, *Hausurnen*, Berlin, 1924.

¹⁷ U. Rellini, *Scavi preistorici a Serra d'Alto*, *Not. Scav.*, 1925, Plate XVIII, 1, 2.

¹⁸ Gordon Childe in *ANTIQUITY*, XXIII, 1949, 135.

What *matters* in Archaeology?*

by R. E. M. WHEELER

I SUPPOSE that everyone now and then sits back, whatever his occupation, and asks himself or herself the question: 'Is what I am doing worth while? Whither am I going? Is my journey really necessary?' As often as not, we muffle the answer and indulge in a comfortable sophistry. Save in those restful operations, such as war-service, where we have surrendered our personality and merely march in step, we are rarely honest with ourselves. We live largely, I am afraid, on self-flattery and evasion. Our life is but a sleep and a forgetting. As scientists, for example, we may say that *all* scientific knowledge, *all* scientific discovery, is worth while, and whatever we may be doing as scientists is *ipso facto* justified. Science is an end in itself.

If we argue in that fashion, we are of course evading the point. Everything we do in life is selective. We select our boots and neckties; within narrow limits we select our lunch and our residence. As I look across this room, my eyes are instinctively selective: at the present instant, they have selected the third person from the right in the fourth row from the back. As scientists, our life is founded on selection and decision. We like to think that that selection and decision are objective and impersonal. What fools we are!

For let us never forget that the subject of our particular science is Ourselves. We falsify our approach at the outset by calling ourselves *Homo Sapiens*. We then proceed to demonstrate what a fine fellow *Homo Sapiens* is; how age after age he has been stimulated by opportunity or adversity from triumph to triumph, from revolution to successful revolution; from hand-axe to atomic pile. Even if the task means sitting down as authors to 12 volumes of typescript, we do not shrink from it. True, the Chinese set a term to this sort of thing: 'All the wisdom of the world', says the Chinese proverb, 'is contained in a cartload of books'. But so much moderation is the exception to the rule.

As archaeological scientists, our subject is Man, a subject which, being Men ourselves, we can never fully objectify. Our science is of all sciences the most subjective and selective. Within the far-off boundaries of ultimate causes, the geologist can comprehend and objectify his stone; a simple and obvious natural law prevents the human mind from an equivalent process of self-comprehension. We obviously cannot be inside ourselves and outside ourselves simultaneously. However scientific certain of the mechanism of archaeology may be, archaeology must remain itself essentially an inexact science, subjectively selective.

I do not propose, however, to develop this semi-philosophic aspect of our discipline this afternoon. My purpose is to take more specific problems, with, I hope, a more immediately practical trend. I have asked the question—and please note the question-mark—'What *matters* in archaeology?' not as a philosopher but as a working archaeologist. I do not expect to answer that question, but can console myself with the thought that questions are often more valuable than their answers.

Our subject is Man, and I repeat that truism because it is the only easy thing we can say about archaeology. Beyond that point, paths diverge with a bewildering complexity. What do we mean by 'Man'? The amiable Dean Swift described him as 'a forked straddling animal with bandy legs'. The greatest philosopher of clothes, following the Dean's tradition, regarded Man as 'a forked Radish with a head fantastically carved'; though in a rare moment of charity that same philosopher conceded that he was 'also a

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Spirit, and unutterable Mystery of Mysteries'. That definition may have thrown dust in the eyes of Chelsea in the 1830's, but it no longer satisfied German realism a generation later. 'Man is what he eats', said Feuerbach; and I am inclined to think that there is a great deal more in that definition than might appear at first sight. The man, for example, who fills his belly with rice is a different species, almost a different genus, from the man who fills his belly with wheat. His whole reaction to the problems of life is a world apart. But let us nevertheless brush aside Feuerbach's definition as a heavy Teutonic witticism and turn to another statement of German materialism. It takes the form of a protest against those idealists who believe that man comes trailing clouds of glory from God which is his home. It reads: 'Do these gentlemen think that they can understand the first word of history as long as they exclude the relations of man to nature, natural science and industry? Do they believe that they can comprehend any epoch without grasping the industry of the period, the immediate methods of production in actual life? . . . Just as they separate the soul from the body, and themselves from the world, so they separate history from natural science and industry, so they find the birthplace of history not in the gross material production on earth, but in the misty cloud-formation of heaven'. Or, to quote again from the same thinker: 'In changing the modes of production, mankind changes all its social relations. The hand-mill creates a society with the feudal lord; the steam-mill a society with the industrial capitalist. The same men who establish social relations in conformity with their material production also create principles, ideas and categories in conformity with their social relations . . . All such ideas and categories are therefore historical and transitory products'. As my listeners may have guessed, I have been quoting from the writings of a certain Karl Marx who, I understand, was a continental archaeologist of the Gladstonian era. In all this there is of course a lot of the old problem of the relative priority of the hen and the egg, to which are added all sorts of complications arising out of chicken-feed. The whole thing is a tangle, and it is to me sometimes a refreshing thought that at the end of his life Karl Marx was himself moved to proclaim 'At any rate I am not a Marxist'.

Well there we have it. Man is (a) a bandy-legged animal, (b) a bagful of star-dust, (c) a Frankenstein victimized mentally and socially by his own handiwork. There are probably 101 other definitions but these will suffice. I have no doubt that they are all true. But you see how difficult this diversity makes things for us! As a Council for British Archaeology we are interested in policy. Indeed, with the active help of Professor and Mrs Hawkes, Professor Piggott and others we have produced a most brilliant statement of present achievement and future needs, and I hope that we shall produce many more. The statement of present achievement will retain a permanent place in our archaeological literature. Its corollary, the statement of future needs, is a delightful and entertaining *jeu d'esprit* but doubtless as evanescent as its predecessors in the past decades and its successors in time to come. However we may try to steer the future, the archaeological biologist will calmly continue his evaluation of Man in the jellyfish-to-vertebrate succession; the idealist will continue to discover spiritual qualities in Romano-British sculpture; the materialist will scrape the pollen off his fish-hooks and wonder why the barb was placed outside rather than inside. Upon the gates of the future we men of the past may hammer in vain.

All this may sound a little disheartening, but I at least am not disheartened. Having in my last paragraph cut the ground away from beneath my feet, I propose to hitch my wagon to a star and carry on. In all this welter of aim and method, is it useful or even possible to say what matters more and what matters less? I remember hearing a distinguished pre- and proto-historian, who may or may not be present here this afternoon,

remark that medieval archaeology in this country was 'a worked-out mine'. I remember also how on one occasion in this room in my youth I got up to read a carefully prepared paper on a new prehistoric beaker which could not fail, I felt, to make a deep impression on the Fellowship before me. As I began, a husky voice came from the midst of my audience, uttering, in terms of withering scorn which I cannot simulate, the words '*More Beaker-folk!*', followed by much scuffling as the owner of the voice struggled for the door. Well, there at once we have two points of view as to what does or does not matter. To this aspect of the problem I shall return. Again, there is the interesting question as to *standards of research*, particularly of excavation. As a young student, I was briefed by a former President of the Society of Antiquaries before undertaking a certain piece of research, and I remember his saying to me 'Of course you mustn't pay any attention to Silchester—it was dug up like potatoes'. And a few weeks ago, 5000 miles from here, I was sitting on the red-hot brickwork of a famous prehistoric city in the Indus Valley with a young archaeologist fresh from Oxford, after checking up with him certain published plans of previous excavations on the site. Everything that we had checked had been found wrong—wrong in detail, often wrong even in general sense. He was properly scornful, and his scorn was of a kind to which I have myself in fact given expression on other occasions. At almost every point our predecessors had lacked, and notably lacked, that degree of scholarship which, since Pitt Rivers began his work in 1880, has constituted the minimum standard of archaeology in the field, and is even more vital than scholarship in the study because normally it cannot afterwards be corrected—the evidence being largely destroyed, of course, in the process of discovery. Nevertheless, I was moved to take up for a moment the defence of these unscholarly folk, and my argument is relevant to our problem this afternoon. It is all a question of *What really matters*.

Let us for a moment consider the situation as it originally confronted the excavators of Silchester on the one hand and Mohenjo-daro on the other. First, Silchester. In 1890 none of our Romano-British towns had been excavated; we knew almost nothing of their plans and buildings, of their material and sociological make-up. We could not, in fact, begin to discuss the economy and sociology of Roman Britain. What was wanted, and within a measurable space of time, was just such a picture as the excavators of Silchester proceeded to give us. The picture was of course both synthetic and incomplete; but thereafter we knew certain fundamentally important things about our Roman towns and could begin to fit them into our general scheme. It would have taken ten Pitt Riverses ten times as long as Pitt Rivers's two decades to have dug the site accurately in depth, and meanwhile much else that was now due would have been kept waiting: the whole accelerating progress of Romano-British studies would have been held up, the impulse lost. It is a sound military axiom that a second-rate plan carried out in time is preferable to a first-rate plan executed tardily; and, applied with caution, this axiom may be apposite on occasion to scientific research. It was certainly apposite to Silchester, however little the matter may have been understood by the excavators themselves.

Similarly in respect of Mohenjo-daro. Before 1921 the very existence of the most extensive riverine civilization of the ancient world was unknown. Then the discovery of a few seals and potsherds and brick walls hinted at a new world. Now what would have happened had you or I been concerned with the matter? I think we can guess. We should have dug a number of careful sections. From them we should have abstracted a reasoned grammar of Indus pottery, beads, seals and implements. We should have uncovered here and there a few buildings which might or might not have been intrinsically

important. We should, in short, have produced an adequately documented Indus Valley Culture.

But what in fact did the excavators do? Practically none of these things. After 12 years or more of work on an extravagant scale, we still have no notion of the development in depth of the Indus Valley Culture. We know almost nothing of the development of Indus Valley pottery or seals or other equipment. We are told that age after age this remarkable culture preserved an uncanny uniformity. The reason is of course that the excavation was almost completely unanalytical. Stratification simply was not understood. All the colours in the paint-box were mixed up, producing a uniform muddy brown.

But what the excavators, with all their up-Guards-and-at-'em methods *did* do, was something that in a sense transcended all this. They built barracks on this desert spot, imported whole regiments of hillmen with their families, and turned two thousand of them or more on to the dusty mounds. Day after day, building after building emerged from the soil, streets and lanes fell into place in a great town-plan, wells and drainage on a scale altogether new to knowledge began to build a picture of an ordered polity that was in the fullest sense an evolved civilization. The excavators found a culture and left a civilization. They did many stupid things, many criminally unscientific things, their records are often childish to a degree, and even within their own limitations they did not fully appreciate what they were finding. I need not go into the details of their sins of commission and omission. As Tacitus said of Julius Caesar's venture in Britain, they may be said to have *shown* the Indus Civilization to us, certainly not to have *delivered* it to us. But they at least did that, as you or I would probably *not* have done. It was all very scandalous, but we have to admit that they, like Schliemann, added a chapter to the history of civilization where we, the grandchildren of Pitt Rivers, might have added a paragraph to a catalogue of cultures. In that particular phase of knowledge, they gave us what primarily *mattered*.

I am well aware that, in applauding Silchester and Mohenjo-daro, I am trespassing dangerously beyond the borders of scientific morality. I am commending crime because it happened to be successful. Let me make it clear that in neither case, either at Silchester or at Mohenjo-daro, am I suggesting that any special merit accrues to the excavators for the methods and policy which they adopted. In fact they knew no better. They had, as I have said, no understanding of digging in depth, of stratification, of analytical excavation. I remember one of them visiting me at Maiden Castle, when, in that closely interleaved site, I was digging a small area with a teaspoon. 'What you want', said my visitor robustly, 'is 300 men to whip the whole of the surface off'. Years afterwards, standing on the eviscerated mounds of his Mohenjo-daro, I recalled his words with a new understanding of his mind.

Nevertheless, hidden away in all this immorality is a moral which is worthy of our attention. I have mentioned Maiden Castle, and am reminded that, in publishing our work there, I expressed regret that circumstances had prevented us from recovering any considerable portion of the ancient town-plan of the place. I think I suggested Hod Hill as likely to be more suitable for a project of this kind. More recently, the authors of our own *Survey and Policy of Field Research* have, I am glad to see, returned to the charge with the remark that at least one Early Iron Age site in each main region of the country, *excavated totally*, 'will now be of more value than merely to sample a number of sites in the same time instead'. True, on the following page they also recommend selective excavations, but the main point stands. We do in fact need more *complete* pictures of our ancient habitations, villages and towns: more long-term work. And in other countries the need may be described as desperate. At the best, we can only hope to find a

disjointed fragment of man's achievement. Let us at least ensure that that fragment is as nearly an entity as circumstances permit. We cannot, I am afraid, pretend that a mesolithic hearth in Surrey or an Iron Age hut in Dorset has the same magic property that Tennyson ascribed to his notorious 'flower in a crannied wall':

' Little flower—but if I could understand
What you are, root and all, and all in all,
I should know what God and man is '.

The mere archaeologist requires something more substantial on which to base the cosmos. 'Those who believe', says the wise Polybius, 'that they can obtain a proper general understanding of history from a study of its fragments are much in the position of those who might think that they could comprehend the life and beauty of a living thing from the *disjecta* of its once living body'. If we must have fragments, let us at least enlarge them to the best of our ability. There is at present a tendency in our archaeology to *fidget* too much—doubtless a legacy of War but none the more desirable for that. What we want now is more solid, steady, big-scale work; carefully trained for, planned for, and carried through to a logical conclusion.

How is this to be effected? In this country, even after 5 years of peace, there is no insuperable difficulty, provided that the determination is present. There is certainly no insuperable *technical* difficulty. In Asia, where so much remains to be done on some of the main arteries of human achievement, the problem is less straightforward. On a relatively small scale, I have just had experience of it, and there is one technical difficulty which calls for nice adjustment. Setting aside the summary methods to which I have referred, we are presumably left with the deliberate scholarly methods of the Pitt Rivers tradition. But, for the reasons already given, these methods, undiluted, will not as a rule meet the Eastern problem. There the accumulation of soil and other material is on the average five times as great as in this country, and if a reasonable amount of horizontal excavation is to be accomplished—if we are to uncover enough of the plan to *matter*—the vertical digging must be speeded up. For example, at the end of my first month's work in Pakistan this year, I sent back 12 bullock-wagon loads of *selected* pottery to base, but still had no idea what I was digging. This was not a bit like Cranborne Chase, and we (Mr Leslie Alcock and I) had to evolve under duress a system of digging which would not, I hope, be practised at Dorchester by the Oxford University Archaeological Society. As we proceeded from layer to layer we had to make a quick decision on the spot whether seriously to stratify or not; and I have come to the conclusion that, in circumstances such as those in which we found ourselves, this procedure is necessary and right. Had we adopted a more leisurely method and stratified everything, we should have accumulated an entirely unmanageable mass of nominally documented material from strata of which we could not adequately discern the significance, and we should have been little the wiser. We still had, as I say, no notion what we were digging up. It was only within the last 9 days of our work that the meaning and relationship of the immense structure on which we had landed suddenly revealed itself. Had it not been uncovered almost in its entirety, it would have remained a mere shapeless and meaningless accumulation of brickwork, unrelated to the citadel on which it stood and adding nothing, literally nothing, to our knowledge of the civilization to which it belonged. As it was, by limiting ourselves to selective stratification we were able to determine all this and at the same time to secure vital groups of pottery without unduly exceeding our capacity for coping with them. I think we may fairly say that we made the best of both worlds and that our methods produced what *mattered*. But we had throughout a constant and painful struggle with our Pitt Riverine consciences.

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Well, there it is. I find myself, in this year of grace, praising the ravishers of Silchester and Mohenjo-daro and recommending *selective* stratification—all in the endeavour to get *what matters* out of a site at the expense of mere pedantry. After all, if we may dig selectively in the horizontal plane, we may surely dig selectively in the vertical plane. Dangerous advice. For who really knows what will *matter* as knowledge advances ? The whole basis of the work of General Pitt Rivers was that we do *not* know and must therefore record *everything*, however seemingly insignificant at the time of record. And Professor Hawkes's recent re-assessment of some of the General's evidence has shown us how right the principle was. But it was right *in the context in which the General was working* : on small sites representing relatively unevolved and reluctant cultures. There the archaeological bow-and-arrow was an appropriate weapon. Against an evolved and embattled civilization a more effective armoury is required : something short, indeed, of the blind bombing of a Schliemann, a St. John Hope or a Mackay, but something certainly including the use of high explosive. The major interrelationships of an elaborate society matter to us a very great deal ; indeed they are a primary concern to us ; and they will only yield to an all-out attack on a broad front. Later, there will be the mopping-up process, which can and should be carried out deliberately with careful small-scale work. In these minor operations British archaeology has in the past been at its best. The time has come now, I suggest, to go beyond this : to apply to big-scale operations the same skill and persistence which we have devoted to small ones. And this we can only do adequately if we remember that the big-scale operation is not merely the small-scale operation writ large. In important respects it is a different sort of manoeuvre. We proudly proclaim the Pitt Rivers tradition but do not let us make a fetish of it. Were the General alive today and confronted with the relics of a great central civilization instead of the smudges of his peripheral slum, he would, I have no doubt, echo fervently the words of Karl Marx and say ' At any rate I am not a Pitt Riversist '.

I appeal, therefore, for a higher measure of concentration, on the part of British archaeologists, upon the *major* achievements of Man as a social animal, and for the evolution of the necessary techniques for the purpose. It sometimes seems to me that we are constantly starting the race in fine form, but running out about half-way down the course. Let us for a change try to come in over the last jump. There is no sort of doubt that we in this country can supply the best initial field-training in the world. Grahame Clark's epoch-making work at Seamer, the annual forays of Ian Richmond and Eric Birley upon a long-suffering but still slightly resistant Hadrian's Wall, set a standard of scholarship scarcely approached by the best of our Continental contemporaries. Cyril Fox has revealed and lit up the mumbo-jumbo of Bronze Age burial with the fires of an unresting imagination. Must all this illumination be hidden perpetually in our ultimate mists ? I was standing not long ago upon a tumult of mighty mounds whither age after age came men from China, from the Mediterranean, from Ind, to exchange their goods and fertilize their ideas, to express the ancient world socially and aesthetically in the most complex polity then known to man. Could we but transfer a tithe of our scholarship for a few seasons to this great workshop of civilization—or indeed to many others of the same high potentiality—what should we not gain in our knowledge of human achievement ? What could *matter* to us more than that ?

For I am one of those elderly persons who still believe in that archaic phrase, the Nobility of Man. I do not close my eyes to Man-the-Jellyfish or Man-the-Whole-time Food-gatherer ; but I believe above all in Man-with-Time-to-think-between-Meals, in Civilized Man. Perhaps as an archaeologist I am merely taking the line of least resistance ; perhaps I am choosing, or attempting to choose, the kind of Man who had most

to say and could express it most amply in material things and is therefore the most susceptible subject for the archaeologist. But I think not. I had already written these words when I heard Professor E. L. Woodward saying much the same thing, though in better shape. He was speaking of certain scholars who had regained for history a place among the Muses, affirming that they did so 'ultimately because they set a high value upon the dignity of man'. 'I repeat this term deliberately', he added, 'because one of the signs of disintegration in our own culture is an unwillingness to consider that man has dignity, and that his acts may be noble. Once this conception of nobility is lost, history becomes nothing more than a rag-bag, a pawnbroker's catalogue, or at best a psychiatrist's case book'. I shall have a little more to add about that in conclusion, in a few moments. Meanwhile there is one more aspect of the matter which I offer for brief consideration.

I referred just now to Cyril Fox's reconstructions of burial-rites in Bronze Age Wales. Fox has recently assembled these reconstructions in the Chadwick memorial volume, and they make at the same time impressive and depressing reading. His account leaves us with the picture of the author we know so well as a man possessed of a trained imagination, of a kind which approximates to genius, pouring his soul into the revivification of the squalid memorials of creatures who had left civilization as far behind them as the Atlantic would (at that time) allow: creatures subsisting on the ultimate foothold of savagery. They scarcely deserve so much flattery; but it is not perhaps too much to say that Fox has almost created a new Fennimore Cooper tradition, that of the Noble (Bronze Age) Savage. He has at least set this savage upon his feet, even though but to bury him again. I remember how, many years ago when the sun was in the East, Fox and I were trudging across a desolate Welsh moorland and came upon a small barrow set within an earthen circle. Offa's Dyke came steadfastly up to the lip of the circle and then on the other side started off again with equal determination on a new alignment. The whole scene stirred Fox's ready enthusiasm, and a week or two later he had dug himself well into the landscape. The mound had by now vanished, and Fox stood, in the spirit, amongst its makers. He was almost physically present at the living ritual, the actual procedure of burial. I quote his own words in retrospect: 'Under the centre of the mound was a deep and large grave-pit on the floor of which lay the skeleton of a full-grown man. To enable this grave to be entered with ease and dignity a sloping passage from ground-level had been cut on the north side. Surrounding the grave-area was a circular trench, which also had a sloping entrance, and on the same side. *But it had no exit*: the area round the grave was isolated. The conclusions drawn from these facts were that the dead man's home was on the north side of the site chosen for his burial; that he had been ceremonially borne by friends or kinsfolk up to, and into, the trench: that those who carried him were not allowed to enter the consecrated area round the grave, but that the persons charged with the performance of the burial rites were awaiting the bearers beyond the trench. The body was handed over, and these persons descended with it into the grave'.

In the present context what matters to us in all this is not of course the particular episode but the creative act of imagination that has gone to the making or re-making of it. Individual after individual, learned society after learned society, year after year, we are prosaically revealing and cataloguing our discoveries, and are excessively content. We dig up mere *things*, forgetful that our proper aim is to dig up *people*. It is *they* that matter, not their pen-knives and their trouser-buttons. We justify our dehumanization of the past by attaching an almost fanatical virtue to what we call our *objectivity*. As so often we are thereby merely elevating our shortcomings into a principle. The only thing that really matters in our work is the *re-creation of the past*. For that we need

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something more, a great deal more, than a dog-Latin transcription of observed data. That is the anatomy of history; but the anatomy is meaningless without its vital interpretation. It is not enough that we archaeologists shall be a variety of natural scientists; we are, and don't let us forget it, above all, humanists. As I have said somewhere else, one sometimes detects today a tendency to devolve archaeology into a dehydrated humanism, to mummify the past, to transform our predecessors into 'battle-axe folk' or 'beaker-folk' until, by a further instinctive reaction, we begin almost to personify battle-axes or beakers with a sort of hungry latter-day animism. I will not, I trust, be misunderstood if I regret this tendency. However broadly we use the words, man is in some sense the casket of a soul as well as five-shillingworth of chemicals. And his recorder must therefore be a good deal more than a rather superior laboratory-assistant. He must also be something of an artist. 'Historical understanding', recently remarked the Oxford historian whom I have previously quoted, 'is more than a series of detective tricks. It requires a mind already attuned to the scale of human action and practised in the subtlest use of language to express the depths and heights'. Those words of Professor Woodward's were well and timely spoken. The historian must have a spark of the comprehending imagination which inspires the painter or the poet. All great historians have this, and owe their greatness no less to it than to their scholarship. They make the past *live* because they are themselves alive and can integrate their reasoned facts with the illogicalities of life. A dangerous thing, undoubtedly, this integration of Reason with Rabelais; but without it we are mere cataloguers, adding dust to dust and ashes to ashes.

I have dwelt upon this matter, not because it is anything new, but because it is very relevant to certain present-day tendencies. After the war-interval I have again during the past few years come into contact with the younger generation of archaeologists, young people under thirty. And I have found in them a change which is due only in part to the impact of the war. Bluntly, as humanists or potential humanists, they are as a whole less well educated, less alert to matters of the imagination, than their predecessors. I am of course getting old, etc., etc., but I think, I fear, that what I say is true. 'Greats' and its equivalents are on the slippery slope. In ten years' time Greek will be to all intents and purposes dead, and Latin will be having a bad pain. These facts are significant pointers. What is replacing the traditional disciplines? A great variety of skills and techniques, but comparatively little of the kind that can educate and stimulate the historical imagination. For this vital, over-riding quality is, as I see it, only in part innate. It is liable in most of us to be a tender growth and needs careful nourishment. It needs the sort of nourishment that reflective literature of the highest quality, with a historical bias, can best supply. It needs something equivalent to a classical education, and I am not quite sure what the suitable equivalents are. Whatever they be, they are urgently necessary if we are to save archaeology from the technicians.

For the second time during these discursive remarks I listen to myself with a somewhat pained surprise. Throughout my life I have done my little best to emphasize the technical needs of archaeology. I even went so far as to found an Institute where these needs could in some measure be supplied. Perhaps again it is a symptom of old age that, having tried to spur the horse to a gallop, I am now hanging on to the reins to prevent it from running away. Well, so be it.

And now lastly. I have pleaded for wider horizons, I have urged the importance of the comprehending imagination. But whither, at the best, can all this lead us? How far can we hope to discover and recognize that which matters, as distinct from that which is merely new? What phases, what regions have really *mattered* in the story of human

achievement? The other day I rashly suggested to a Palestinian archaeologist that Palestine was rather a backwater. 'Yes', he replied modestly, 'but, after all, we *did* produce the Trinity'. The conversation then lapsed. I have since been wondering, as every archaeologist has wondered sometime or other, what the value of our evidence really is. What do our bits and pieces amount to? Listen, if you have courage, to the grave words which Dr John Donne uttered on this subject three and a half centuries ago:

'The ashes of an Oak in the Chimney are no epitaph of that Oak, to tell me how high or how large that was; it tells me not what flocks it sheltered while it stood, nor what men it hurt when it fell. The dust of great persons' graves is speechless, too; it says nothing, it distinguishes nothing. As soon the dust of a wretch whom thou wouldest not, as of a prince whom thou couldest not look upon will trouble thine eyes if the wind blow it thither; and when a whirlwind hath blown the dust of the Churchyard into the Church, and the man sweeps out the dust of the Church into the Churchyard, who will undertake to sift those dusts and to pronounce, this is the Patrician, this is the noble flour, and this the yeomanly, this the Plebeian bran'?

Who indeed? Not the poor archaeologist, who may at best bottle the dust and send it hopefully to Professor Zeuner. Dr Donne cannot be gainsaid, but see how he cuts at one of the roots of our discipline! A Greek (was it Thucydides?) somewhere remarked that a great nation may leave behind it a very poor rubbish-heap. And are we, as practising archaeologists, to award the palm to the unknown Sumerian who was buried at Ur with 63 helmeted soldiers, grooms and gold-garlanded damsels, two chariots and six bullocks, or to the Nazarene in a loin-cloth who was nailed up on Golgotha between two thieves? I merely ask the question, but cannot help feeling that, were archaeology alone the arbiter, the answer would not be in doubt. Give us helmets and gold garlands every time; bread and circuses give us, provided that the bread is carbonized and the circuses well-furnished with good solid bronze and marble. But let us at least, in our gratitude for these things, remember the missing values that cannot be appraised in inches or soil-samples or smudges in the earth.

All this is not very encouraging. The archaeologist may find the tub but altogether miss Diogenes. He must accept that risk, consoling himself with the reflection that no single approach to human accomplishment can be other than partial and chancy. The literary historian who overlooks art and craftsmanship and environment may lose as much as the archaeologist who can produce for us a harp without its music or a tub without its philosopher. Let us therefore count our blessings. We cannot read the language of the Minoans, but their palaces and frescoes, their wares and jewels, are themselves a pictographic language that tells us not a little of their way of living, and hints, however vaguely, at their way of thinking. We must do what we can with the material vouchsafed to us, in full consciousness of its incompleteness. But try we must, always with the thought that archaeology and history are alike frustrate unless they contribute to a vital *reconstruction of man's past achievement*, in other words aspire to interpretation as well as to mere transliteration. Sir Arthur Evans was a thousand times right in his interpretative attempt to reconstruct the Palace of Knossos, after faithfully recording the evidence as he found it. Mrs Hawkes and her colleagues were right in their attempt to reconstruct Little Woodbury and so, of course, was Cyril Fox in his reconstruction of a British chariot. The task of reconstruction, whether three-dimensional or two-dimensional, is one from which the archaeologist must not shrink. It is the crown of his work. And it is surprising and reassuring to find how much good constructive material can in fact be extracted from a rubbish-pit—or lie implicit, for that matter, in the tale of a tub.

Basilica Discoperta

by A. M. SCHNEIDER

THE question of origin and pedigree of the Christian basilica has occupied scholars again and again, but no generally recognized result has been achieved so far (1). Recently Ejnar Dyggve has put forward the thesis that the old Christian basilica is the final result of a long development which started with the classical Heroon. Dyggve himself has excavated such a Heroon at Kalydon and has published a fine report on it (2). It is the tomb and sanctuary of the chief of a clan, Leon, who was regarded by his descendants as a new Heracles. The building consists of a peristyle with a transept added in front of the north side which opens into a square sanctuary (FIG. 1). In the sanctuary is an altar and at the rear wall a base on which stood presumably the statue of the deified dead. The sanctuary was closed in front by a balustrade. Underneath it is a room serving as tomb with two beds of stone; this room was accessible from outside. About the same time a very similar building was discovered on the citadel of Pergamon (FIG. 2), the only difference being that there was no burial vault underneath the sanctuary, but only a shaft reaching down to the virgin soil which probably once contained funerary urns. The excavators (3) take it to be a monument dedicated to the cult of the rulers of Pergamon—which means that we have here again a Heroon. These two similar structures invite the guess that we have to deal here with a widespread type and this interpretation is corroborated by another building in Kalaureia (4). Dyggve thinks that this type of structure is the prototype of the Christian basilica of the martyrs and not only of this but of the Christian basilica altogether, which consists also of a nave with rows of columns with sometimes a transept to which is joined the apse with altar and *confessio* of the martyr. This derivation seems to be confirmed by the discovery of another Christian sanctuary at Marusinac near Salona which forms, so to speak, the connecting link. There Dyggve (5) found a vaulted mausoleum with an altar under which was a burial vault with the coffin of Anastasius who was executed in A.D. 304. In the course of the 4th century a Christian cemetery grew up round this martyrion which was destroyed in 395 and then overbuilt with a bigger structure (FIG. 3). East of the martyrion is a sepulchral basilica of the usual shape. The relics of Anastasius were translated into this building and buried in a small *confessio*. North of it is a second building consisting of an apse, two transepts of the type of the *liwan*, and a colonnaded open court with tessellated floor. In the roofed-in parts are tombs of bishops and other persons of social standing. We have therefore here a type of building somewhat similar to the pagan Heroon. Dyggve thinks that this type of building represents the *basilica discoperta*. He takes this term from an anonymous pilgrim of Piacenza who visited the burial temenos of the

¹ I mention only the relevant publications and papers by Zestermann, K. Lange, F. Witting, Le Roux, Bréhier and R. Schultze.

² E. Dyggve—F. Poulsen—K. Rhomaïos, Das Heroon von Kalydon. Kopenhagen 1934.

³ E. Boehringer—F. Krauss, Das Temenos fuer den Herrscherkult. *Altertuemer von Pergamon* 9 (1937).

⁴ G. Welter, Troizen und Kalaureia, Berlin 1941, 51 and plate 44.

⁵ *Forschungen in Salona* III. Wien 1939. E. Dyggve and R. Egger, Der altchristliche Friedhof Marusinac. Dyggve's views with regard to the Basilica Discoperta are elaborated in *Atti del IV Congresso Internazionale di Archeologia Cristiana I*. Roma 1940, 391 ff. and in *Zeitschrift fuer Kirchengeschichte* 59, 1940, 103 ff.

Old Testament patriarchs at Hebron towards the end of the 6th century and left us a description of it. The evidence for reasoning that the basilica of the martyrs of the time of Constantine is descended from the Heroon of classical times seems to be complete.

On the other hand, Theodor Klauser (6) has already pointed out that an apse occurs also in other connections than in rituals connected with burials. He has argued further that the transept is only loosely connected with the peristyle court, and that the transept

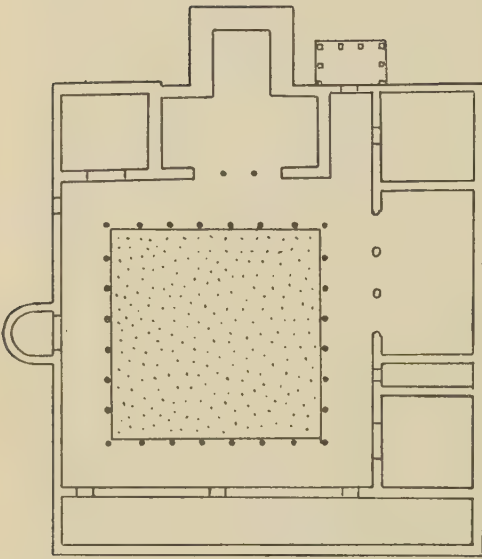


FIG. 1. KALYDON, HEROON. Scale 1:565

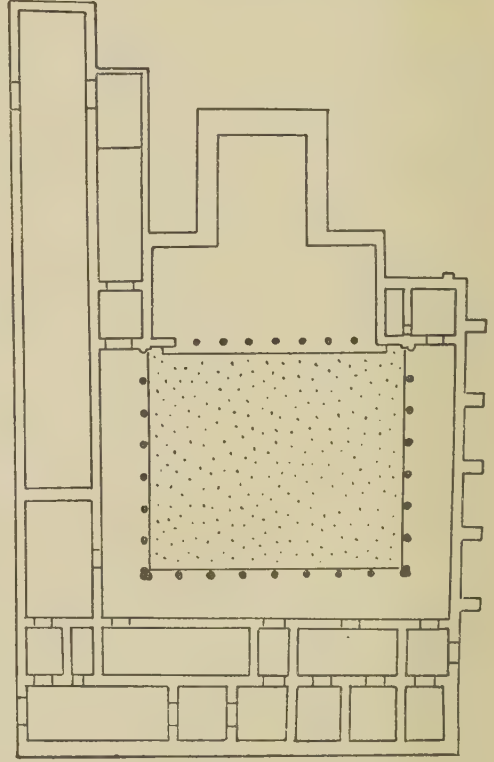


FIG. 2. PERGAMON, HEROON. Scale 1:545

of the Heroon does not offer a convincing analogy to the transept of the basilica. His main argument against Dyggve's thesis is the fact that the oldest basilica with transept and apse of the time of Constantine, the Lateran basilica built 313, was a church for regular assembly *without* a martyr's tomb. As far as we know, no bigger basilica intended for the cult of martyrs had preceded it which could have served as model for the Lateran basilica. Furthermore, the custom of depositing relics in the altar grew up only in the course of the 4th century. Klauser is inclined to admit that the architect of the Lateran basilica of the time of Constantine had peristyle courts in mind when he designed the

⁶ Vom Heroon zur Maertyrerbasilika. Bonn 1942. Kriegsvortraege der Rhein. Friedrich-Wilhelm Universitaet Bonn a.Rh. Heft 62.

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nave. I think Klauser is right in his objections and some others may even be added to them. A priori we have to admit that Dyggve stresses rightly the importance of the cult of the dead among Pagans and Christians and especially of the cult of the martyrs among

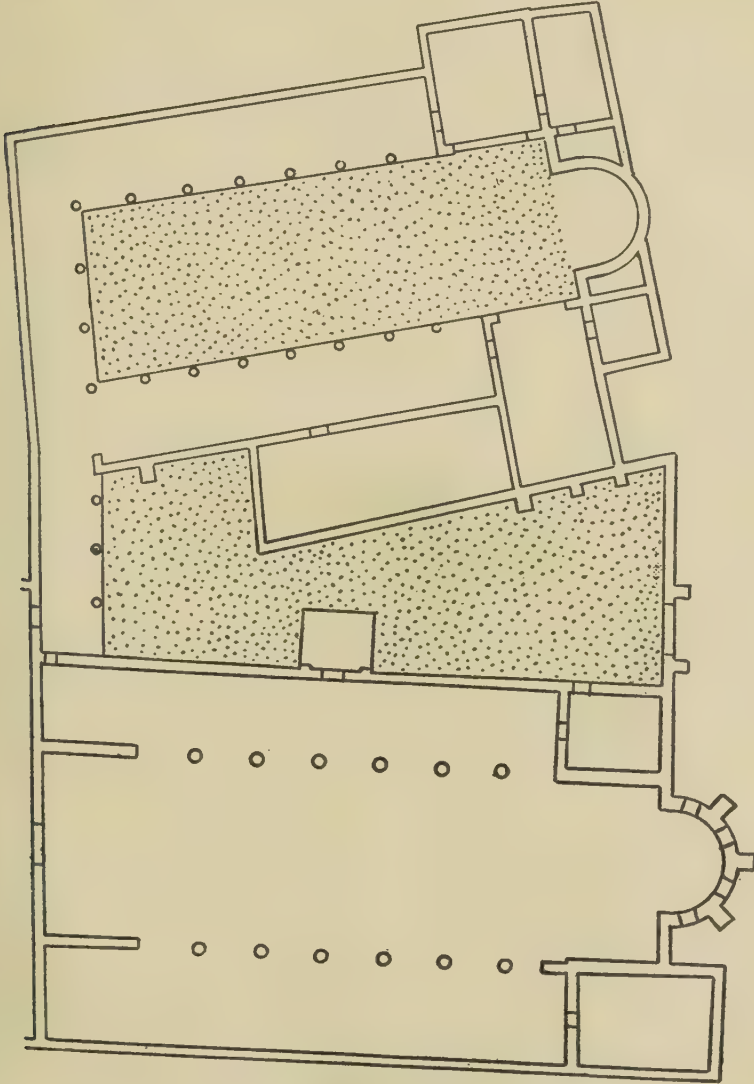


FIG. 3. MARUSINAC. Scale 1:455

the christians. But the cult of the dead and of the martyrs takes place outside the cities and is celebrated only once a year on the anniversary of their death. The ordinary divine service has nothing to do with it; it is performed inside the city boundaries

either in private houses or, after the 3rd century, in specially designed buildings (7), the churches. The eucharist was celebrated from the earliest times (Acts, xx, 8 ff.) in closed rooms (7a) whose entrances were carefully guarded in order that no profane eye should observe. In classical times the mystery rites prefer also closed rooms, whereas other religious ceremonies take place under the open sky. This is not compatible with the assumption that the open peristyle court was the precursor of the Christian basilica, which developed according to the needs of the liturgy of the congregation. Archaeology is not yet able to show all the details of this development, but literary sources and the meagre archaeological evidence provide at least a rough outline. The pattern of the liturgy was already established by about the middle of the 2nd century: it consisted of a service with the reading of a gospel and epistle, and a sermon, followed by the eucharist proper from which the catechumen were excluded (8). The congregation consisted of the laity (strictly separated according to age and sex) and of the clergy who were again separated from the laity and, in medium-sized communities, comprised bishop, presbyters and deacons (9). The laity not only responds to the reading but takes an active part in the ceremony of the eucharist by carrying offerings to the altar from which those items are chosen which are necessary to the liturgy (10). During the communion they receive then what they have brought to the altar as *prospophora*. Therefore a separate room was needed for the altar with the clergy assembled round it, but also sufficient room for the two processions of the laity to the altar. The oldest known private church in a house at Doura-Europos (11) shows at least that the nave was orientated: at the east wall is a slab either for the bishop's seat or for the altar. This arrangement initiates a development as we find it in the basilica of Aquileia built in 313 (12). Here the arrangement of the tessellated floor proves without doubt that we have a transept with altar for presbyters and a three-naved room for the laity. The different panels of the mosaic are presumably intended for the accommodation of the different groups of the laity, separated according to age and sex. In a room like this the ritual of the liturgy as described could be performed according to the underlying idea. Such a room fits the requirements of the cult and has developed without doubt from customary practice. We also know of cult buildings of basilican shape with three naves during classical times. In this

⁷ Ecclesia—Churchbuilding in Tertullian and Cyprian cf. H. Janssen, *Kultur und Sprache* 1938 (*Latinitas christianorum primaeva* 8) pp. 24 ff.

^{7a} The celebration of mass in the open air even today requires the special permission of the bishop.

⁸ cf. Justinus Martyr, *Apologia* I, 65–7.

⁹ The *Didascalia Apostolorum* of the middle of the 3rd century states in this connexion cap. 12 (ed. P. de Lagarde, Göttingen 1911, 56): 'There shall be a separate space for the priests on the east side of the house and the throne of the bishop shall be put up between them; the priests shall sit with him. And on another side of the eastern part shall sit the men of the laity. For it is appropriate that the priests sit with the bishop in the eastern part of the house, then the men of the laity and then the women, so that when you stand to pray, the rulers may stand first'.

¹⁰ According to the *Didascalia* 'one of the deacons should always be present at the *Prospophora* of the eucharist'. This rule only makes sense if he had to regulate the offerings of the believers because he did not need to keep watch on the *korban* (offering). For Africa this offering is vouched for by Tertullian, *Apologia* 39; Cyprian, *Epist.* 28; *de opere et eleemosynis* 15. The relevant quotations in the *Sacramentarium Leonianum* have been collected by Klauser: *Roem. Quartalschrift* 43, 184 ff.

¹¹ C. Hopkins, *Christian Church at Doura-Europos*, Preliminary Report of fifth season of work of the Excavations at Doura-Europos. New Haven 1934, plate 39.

¹² cf. A. Gnirs, *Oesterreich. Jahreshefte* 19/20, 1919, Beiblatt 194, fig. 88.

connection should be mentioned the building for a mystery cult near the Porta Maggiore in Rome (13) and the building for the cult of Serapis at Miletus (14). As we know hardly anything about Pagan liturgies we can only guess that some of them may have resembled the Christian liturgy. This appears to have been recognized also by the Christians, who ascribed this similarity to the influence of evil demons (cf. Justin, *Apologia* 1, 58, 4). Similar liturgical usage produced a similar room, and it is even not necessary to deduce a reciprocal dependence. The Christian cult room is therefore to be regarded as originating solely from the facts of the cult, while it is obvious that for the monumental elaboration of the basilica of the time of Constantine the classical market basilica, and not the peristyle court, provided the inspiration. The market basilica, on the other hand, is a room *sui generis* which had to serve other purposes and was therefore subject to other principles of planning: it is without orientation or at least neutral with regard to orientation. This meant that it opened towards the long sides or towards the short sides according to its local position. But the vital centre of the market basilica is always the middle, surrounded by columns on all sides. When the architects of Constantinian time built the great representative basilicae, they were tied to the already developed scheme which was in plan close to the *basilica forensis*; on the other hand it was the *basilica forensis* which provided them with the necessary means for the architectural elaboration. Therefore it is out of the question that peristyle courts have served as prototypes and it is inconceivable why the Constantinian architects should have taken that roundabout way, especially as open court and roofed-in hall are independent elements of architecture which have always existed side by side and which have not developed one from the other.

Eusebius (15) terms the church building *ἐκκλησία, οἶκος ἐκκλησίας, οἶκος εὐκτήριος* and also *βασιλική* (Vita Const. 3, 52; it has to be noted that the Vita was apparently re-edited and revised in later times), but in *Hist. Eccl.* 10, 4, 42 *βασιλικὸς οἶκος*. This last term proves that the author was not quite familiar with the substantive *βασιλική*. This is not surprising as the basilica was not a Greek but an Italian invention. In Greek *βασιλική* is always used as an adjective (*βασιλική στοά, βασιλῆιος στοά*); it occurs as a substantive only where it is a translation from basilica (16). The Greek knows neither the architectural type—in the Greek market stood the Buleuterion and not the basilica—nor the name. The *βασιλῆιος στοά* in the market of Athens which has been regarded for a long time as the prototype of the basilica looks quite different (17), and must be eliminated as model. Basilica as a substantive is known only in the Latin language, in fact for the oldest known building of this type, the basilica Porcia (about 184 B.C.). Of the older basilicae known from literary evidence, the only one still preserved is the basilica Aemilia which was from the very beginning three-naved as well as roofed-in. If we look for prototypes of the Christian basilica we have to turn, if at all, to the roofed-in market basilica and not to the peristyle court. The peculiar *basilica discoperta* of Marusinac ought therefore to be disregarded in this discussion. But we have to put the question: Where does Marusinac come in, and how is it to be explained? It must be stated at once that *basilica discoperta* is not a technical term. It is found only once in the entire early

¹³ M. Carcopino, *La Basilique Pythagoricienne de la Porte Majeure*, Paris 1927.

¹⁴ Milet, 1, 7: H. Knackfuss, *Der Suedmarkt*. Berlin 1924, 180.

¹⁵ cf. P. Mickle, *Die Konstantinkirchen im hl. Lande* (Das Land der Bibel IV, 3) Leipzig 1923, 30 ff.

¹⁶ First mentioned as far as I know in an inscription in Oropos in A.D. 73; Dittenberger, *Sylloge Inscr. Graec.* Nr. 747.

¹⁷ Groundplan *Hesperia* VI, 1937; plate II.

Christian literature, i.e. in the already mentioned Anonymus Placentinus who travelled in Palestine in 570 and left a description more naïve than lucid (18). In Hebron the pilgrim found the temenos built by Herod I above the tombs of the patriarchs, a walled-in square of 60 by 33 m., the walls of which are still standing today to a height of about 10 m. Of this classical type of architecture the simple pilgrim had no more idea than had the Anonymus of Petrus Diaconus who described it as *ecclesia sine tecto* (19). On the

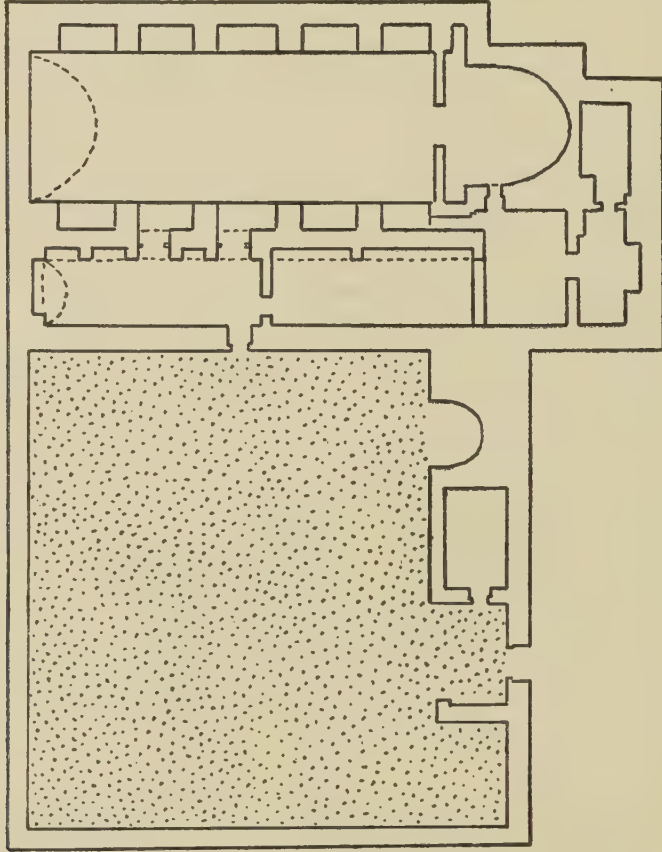


FIG. 4. KEFR ZEH, MAR AZIZAEI. Scale 1 : 243

other hand the man from Piacenza described it in chapt. 30 as follows : *in quo loco . . . basilica aedificata est in quadriporticus, in medio atrio discopertus* (variant : *atrium in medio discoopertum*) *per medio discurrit cancellus, et ex uno latere intrant Christiani et ex alio latere Judaei, incensa facientes multa.* The approximately contemporary recensio B, reads as follows (20) : *Est basilica in quadriporticus, atrium in medio discoopertum et in*

¹⁸ P. Geyer, *Itinera Hierosolymitana* C.S.E.L. 39, 1898, 179 and 209 ; the attribution of this report to Antoninus cannot be maintained ; cf. Grisar, *Zeitschr. f. kathol. Theologie* 26, 1902, 760 f.

¹⁹ Geyer, l.c. 110. ²⁰ Geyer, l.c. 209.

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medio cancellatum . . . (variant: *basilica aedificata est in medio atrio quadriporticus discooperta*). As both versions with their variants prove, no one was able to get a clear idea of the facts from the description. Either the pilgrim saw only four *porticus* running parallel to the enclosing walls or, as recensio B seems to suggest, a basilica with peristyle.

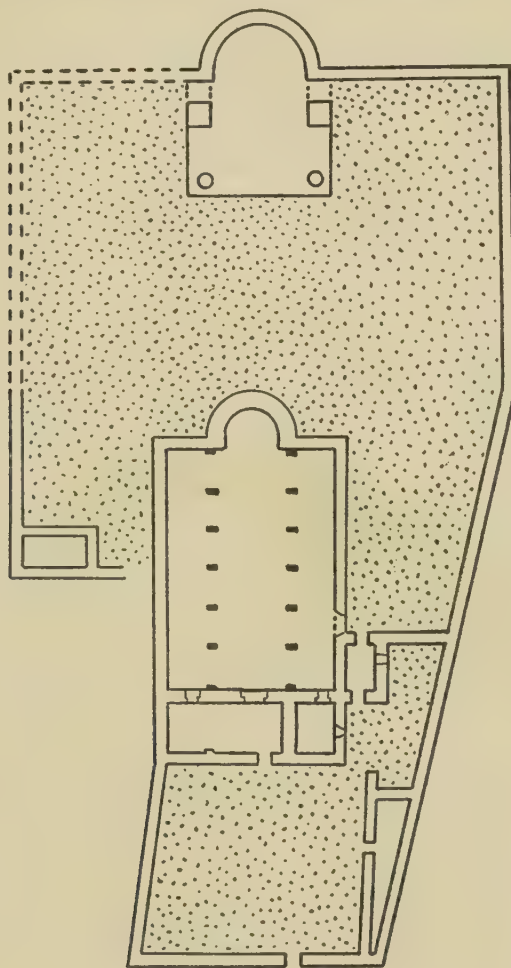


FIG. 5. BIN BIR KILISE No. 7. Scale 1:555

What was in fact there cannot now be decided on the spot. If the latest views of those dealing with the problem (21) are correct, namely that the layout of the crusader church, today a mosque, inside the temenos points to an early Christian basilica, then we should be dealing with a structure such as recensio B seems to imply. In any case it cannot be interpreted as a building like Marusinac nor can Marusinac be regarded as basilica

²¹ H. Vincent—E. Mackay—F. Abel, Hébron, Le Haram el-Khalil, Paris 1923.

discoverta. Such a type of building never existed, whatever interpretation the clumsy text of the pilgrim may suggest. The structure in Dalmatia has to be explained in a different way. Open places of worship surrounded only by a wall seem to have been used in the Jewish cult, i.e. the Proseuche (22). It is possible that the Islamic *musalla* (the place where the *salāt* are performed) belongs to this context, an enclosed open square with a mark giving the direction for the prayers (23). It is used for prayers for the dead, also for prayers for rain. These open praying places are found also in the Christian world, and there is even a name for them known from the Tûr 'Abdîn, i.e. *bēt selôtā*: place of worship. They have been identified in Kefr Zeh (FIG. 4), Hashtarak, Hah, Arnas and Bassibrina (24). They show an open square south of the generally single-naved church with an apse towards the east which is known according to a Syrian inscription on the building as *bēt selôtā* (25). The question now is—what liturgical functions took place there? Miss Gertrude Bell elicited the information that the service was performed there on weekdays (26). This cannot have been the original purpose of the building, as Pognon relates (l.c. 42; 93, note 1) that the monks were having the daily office there during the hot season (27). In fact there is no altar in the *bēt selôtā*, but a *gūdā* (pulpit) on which the books for the prayers of the hours are laid (28). Such stone-built pulpits have their place also in the church immediately in front of the choir: here the prayers of the hours are said when the weather is bad. Where there is no *bēt selôtā* the office is held in front of the entrance of the church during summer where again such pulpits are found. However, this also does not seem to have been the sole and original purpose of this remarkable structure. In the *bēt selôtā* conche of Hah and Hashtarak there are many commemorative inscriptions in honour of the bishops and monks buried nearby in a common burial vault. The epitaphs of Hah date from 1136 to 1296 (Pognon Nr. 65–71), those from Hashtarak from 913–1294 (Pognon Nr. 95–116)—the structure points therefore unequivocally to the cult of the dead. It is probable that the daily office was performed also with reference to the dead brethren (28a). If we turn

²² cf. Zarb, *De Iudaeorum προσευχή* Angelicum 5, 1928, 91 ff.

²³ Handwoerterbuch des Islam, Leiden 1941, 549; I saw such places of prayer at Brussa, Iznik (near the Lefke-gate) and in Constantinople, but they are rare or have been built over in later times.

²⁴ cf. Miss Bell in: M. van Berchem—J. Strzygowski, *Amida*. Heidelberg 1910, 243, fig. 174 and 175 (Kefr Zeh); 249 fig. 184 (Arnas); 255 fig. 195 (Hah, Mar Sovo); cf. also Monneret de Villard, *Chiese della Mesopotamia*, Roma 1940. *Orientalia christ. Analecta* 128, 48. The *bēt selôtā* of Hashtarak was built before 772, the others are younger, but apparently reconstructed as the church of Kefr Zeh belongs to the early 7th century.

²⁵ H. Pognon, *Inscriptions semitiques des la Syrie, de la Mésopotamie et de la Région de Mossoul*. Paris 1907, 93, Nr. 51 (Kefr Zeh).

²⁶ *Amida* l.c. 245. It is to be noted, however, that the Nestorians did not celebrate any liturgy in the early days during the ferial days, but kept only to the hourly prayers (*teshmeshta*): R. H. Conolly, *Anonymi auctoris Expositio officiorum ecclesiae* 2, 4 and 3, 4: *Corp. Script. orient. Script. Syri, Series II*, 91; Versio, pag. 107, 175.

²⁷ With this would correspond the fact that the Talmud knows summer and winter synagogues: b. baba bathra fol. 3b (Goldschmidt 925) and that in Constantinople there exist also summer and winter mosques. The mosque of Mimar Sinan in the valley of Lycus, for instance, shows two buildings side by side; today only their foundation walls are preserved.

²⁸ cf. Pognon l.c. 43.

^{28a} This would fit well with the fact that in Salah (Mar Yakub), though there is no *bēt selôtā*, the memorial inscriptions are existing in the entrance hall (dated from 908 to 1370; Pognon l.c. 62 Nr. 22–33), where also the daily officium took place during the summer.

now again to the Dalmatian building which was also devoted to the cult of the dead, it is clear that this is not accidental. The monks of the 10th century were carrying on with a custom of early Christian time of which the oldest preserved example still in existence is Marusinac. In this connexion it is to be emphasized—and this seems to me very important—that precisely in Salona we know of Christians hailing from Syria (29). The assumption is permissible that in the peristyle court at Marusinac (as far as it was covered with mosaics) meals in honour of the dead took place, especially for the benefit of the poor (30). The court chapels at Korykos (31) and Bin bir Kilise (FIG. 5) (32) which are comparatively near to the Syrian and North Mesopotamian area served probably for the same purpose. The so-called *basilica discoperta* has developed from the perhaps local practice (33) of the cult of the dead and is a special case which cannot be connected with the regular divine service and which, therefore, cannot be worked into the pedigree of the Christian basilica (34). It must remain undecided whether the open place of worship, at home in the Orient and only there justified, was influenced by the Hellenistic Heroon. I do not think it very probable.

²⁹ Salona III, 114.

³⁰ cf. the Commentary on Job, Migne P.G. 17, 517: *Celebramus nimirum religiosos cum sacerdotibus convocantes . . . invitantes adhuc egenos et pauperes, pupillos et viduas saturantes, ut fiat festivitas nostra in memoriam requiei defunctis animabus, quarum memoriam celebramus.*

³¹ G. Bell, *Revue archéologique* 1906, II, 8, fig. 1; 28, fig. 20; Olbia 34, fig. 26.

³² Fig. from K. Liesenberg, *Der Einfluss der Liturgie auf die fruehchristliche Basilika*. Neustadt 1928, 149, fig. 58. Kirche VII.

³³ Perhaps the building at Marusinac can be connected with the curious twin buildings at Salona and Aquileia. Besides the basilica with which I have dealt above stands a second one, but without altar—is this second basilica possibly a room for the cult of the dead? Or a room for agape? It is possible that the custom of the meal in honour of the dead was also known in Mesopotamia. In the Vita of Mar Daniel Asya (Bedjan, *Acta Martyrum et Sanctorum* 3,505) we find the description of a Nestorian church with 'three spacious courts and inner cells for schools and Vigil celebrations'. The word *shahra* can mean Vigil as well as *cena funeraticia*.

³⁴ A. Boethius, *Festkrift tillägnad J. Arvid Hedwall* 1948, 71 f. follows up Dyggve's thesis and quotes as further examples the basilica of S. Sebastiano in Rome as well as the structure in front of Sta. Costanza. But I cannot see how he can prove the case for S. Sebastiano. Sta. Costanza has to be left out too, as the outer wall shows many big windows which would be quite unnecessary if it had been a court basilica. I am also very sceptical when Dyggve thinks that the hypaetral basilica can be identified in palaces of late classical time (*Ravennatum Palatium sacrum*. Copenhagen 1941, 30; 48; 53 *Kgl. Danske Videnskab. Selskab. Arch.-kunsthist. Meddelelser* III, 2). I find it difficult to believe that the area outside the entrance to the palace at Spalato can be regarded as a ceremonial hall. Diocletian built and occupied the palace as *privatus* when he gave no more receptions in his quality as emperor. I see in the arcades no more than relics of the colonnades along a street which lack the accompanying wall. This wall had to be omitted because, if it had existed, it would not have been possible to see the temple and the mausoleum. The Chalke in Constantinople may have been copied by Theodoric, but we have no witnesses that ceremonial receptions took place there; neither does it seem necessary to reconstruct the building at Ravenna as done by Dyggve on plate XII. I see no reason not to assume that the façade as represented in the mosaic of San Apollinare Nuovo stood with a straight line colonnade. Such straight frontal colonnades from which projects the propylon are known; I mention only the façade of the pre-Justinian church of Sta. Sophia at Constantinople which I excavated and the central hall of the camp of Diocletian at Palmyra (Wiegand, *Palmyra* 1932, Textband 94, fig. 106). In Ravenna we would have correspondingly the front of the palace precinct. I have to add that the arcaded street leading to the palace in Constantinople was two-storied: *Cod. Theod.* 15, 1, 45.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

- CHINA'S DISCOVERY OF AFRICA, by J. J. L. DUYVENDAK. Probsthain, London, 1949. 35 pages. 6s.
- THE BUILDING OF HADRIAN'S WALL, by C. E. STEVENS. *Soc. of Antiquaries*, Newcastle-upon-Tyne, 1948. 3s.
- THE CENTENARY PILGRIMAGE OF HADRIAN'S WALL, 4TH-9TH JULY, 1949, compiled by ERIC BIRLEY. Same as last, 5s. [Excellent bibliography].
- THE NORSE STYLE OF ORNAMENTATION IN THE VIKING SETTLEMENTS, by HAAKON SHETELIG. *Acta Archaeologica*, XIX, 1948, 69-113.
- MAN THE TOOL-MAKER, by KENNETH P. OAKLEY. Brit. Mus. (Nat. Hist.), 1949, 2s 6d. [An admirable popular account, giving a full account, with drawings, of types of stone implements and how they were made].
- NEW EVIDENCE OF THE ANTIQUITY OF PILTDOWN MAN, by KENNETH P. OAKLEY and C. RANDALL HOSKINS. *Nature*, 11 March 1950, p. 379. [Derived from fluorine analysis].
- RICHBOROUGH, 4TH REPORT, by J. P. BUSHE-FOX, C.B.E. Research Reports, *Soc. of Antiquaries*, 1949. Quaritch, £2 2s od. [A further instalment of this now classic series needing no commendation here].
- BRITISH PREHISTORY, by STUART PIGGOTT. Home University Library, Oxford, 1949, 5s. [An excellent popular account based on much original work].
- A HUNDRED YEARS OF ARCHAEOLOGY, by GLYN DANIEL. Duckworth, 1950. 21s. [A much needed history covering new ground].
- 32 BERICHT DER RÖMISCH-GERMANISCHEN KOMMISSION, 1942. De Gruyter, Berlin, 1944 (published 1950). [We heartily welcome the reappearance in excellent format of this celebrated publication].
- WILLIAM STUKELEY, AN 18TH CENT. ANTIQUARY, by STUART PIGGOTT, O.U.P. 18s. [More than a biography of S. and very good reading].
- BRITISH ANTIQUITY, by T. D. KENDRICK. Methuen, 21s. [A history of antiquarianism in the pre-Stukeley period and equally readable].
- THE EARLY CULTURES OF NORTHWEST EUROPE: 22 essays written in honour of H. M. CHADWICK, edited by SIR CYRIL FOX and BRUCE DICKINS, C.U.P. £3 3s. [A valuable and scholarly festschrift].
- THE MOA-HUNTER PERIOD OF MAORI CULTURE, by ROGER DUFF. Whitcombe and Tombs. 3-4 Addle Hill, Carter Lane, E.C. 4. £1 12s.

Correspondence

Dear Sir,

We have read with interest the article in your June issue, by J. F. Ewing, entitled 'A new technique for removing bones from limestone breccia'.

For several years we have been investigating the possibilities of the development of fossils from rocks of all geological ages by chemical methods, and feel that some of our findings may be of interest to Father Ewing and to others among your readers. These results are being published from time to time in the 'Museums Journal' (*Mus. Journ.*, 48, p. 54; 49, p. 293), but perhaps we may mention here some points in which our technique differs from that of your contributor.

It has been found that the most satisfactory solutions for removing calcareous matrices from bone are a 20 per cent (by volume) solution of acetic acid, or one of 10 per cent Formic acid. In special cases weaker solutions are used. Acetic acid is cheaper and kinder to the skin than formic acid and is thus in more general use. A mixture of these acids attacks bone and is therefore useless. It has not been found necessary to add any other substance to these solutions and good results have been obtained with a very wide range of specimens, from Devonian Ostracoderms to Neolithic bone and ivory implements and ornaments.

In no instance have we been able to detect damage to bone attributable to chemical action of the acetic acid, but where exposed cancellous bone is infilled with calcite the violent eruption of carbon dioxide may damage the delicate bony tissue. It is then desirable to 'screen' these areas of bone from the action of the acid and polystyrene has been found to be the most effective acid-proof substance to employ. It is applied as a dilute solution in ethyl acetate. Several coats are put on, each being allowed to dry before the next is added. Any excess may be removed subsequently with benzine. The total drying-time is much less than that mentioned in Father Ewing's article for the chlorinated rubber solution.

For comminuted or decayed bones Plenderleith's method of impregnation has been found very effective (H. J. Plenderleith, 1934, 'The preservation of antiquities', p. 19, *Mus. Assoc., London*). The specimen is impregnated with a dilute solution of celluloid before immersion in acid and the thin film of celluloid left will be sufficiently strong to hold the bone fragments together when the matrix is removed, but because of its permeable nature will not impede the action of the acid. The insoluble part of the matrix will remain attached to the bone, but can be removed easily by brushing the surface with a camel-hair brush dipped in acetone.

After all the matrix has been removed the specimens are washed in running water for several hours. They are then allowed to drain, but not to dry. The next stage is to immerse them in an aqueous emulsion of polyvinyl acetate. The strength required is about one third of that supplied by the makers. This impregnation may be done *in vacuo* (*Mus. Journ.*, 49, p. 293), but where vacuum equipment is not available an immersion of from six to twelve hours will ensure adequate penetration. On removal from the emulsion the specimens are placed on a rack to dry. A suitable rack can be made from strips of zinc sheet, on edge: care must be taken to ensure that the bone is in contact with as few edges as is consistent with safety, and that these are oiled to prevent sticking. This treatment ensures that the bone will not break up on drying. The emulsion is transparent when dry, preserves the natural tints of the specimen and does not obscure detail.

Trusting that these observations may be of use to your readers.

We are, Yours faithfully,

British Museum (Natural History).

H. A. TOOMBS and A. E. RIXON.

Notes and News

PESTILENCES

‘THE DATE OF CAMLANN’—AND OF THE PESTILENCE OF THE SAME YEAR

I have read the note by Mr P. K. Johnstone, entitled ‘The Date of Camlann’, in the March number of *ANTIQUITY* (p. 44) with great interest.

The article interests me primarily as a student of the epidemics which ravaged these islands during the 6th century, but the possibility—brought out by Mr Johnstone—of Arthurian chronology also being concerned and possibly clarified adds to the interest of the problem.

There are two entries in the *Annales Cambriae* which are involved, namely :

‘537. XCIII Annus. Gueith cam lann, inqua arthur and medraut corruerunt : et mortalitas in brittannia et in hibernia fuit’, and

‘547. CIII Annus. Mortalitas magna inqua pausat mailcun rex genedotae’.

Mr Johnstone’s final misgiving ‘We cannot be sure that the compiler of the *Annales* did not blunder in assembling his entries from disparate sources’, though true, seems to me to invalidate the whole of his argument—and, indeed, to render the whole of this enquiry of no moment.

Possibly the most pertinent question is the reliability of the dating of the *Annales Cambriae*. The editor of the *Annales* in the Rolls Series, John Williams ab Ithel, describes the three manuscripts (A, B and C) : of these, A. (Harley 3859) is the oldest, and is ‘of the latter part of the tenth or the beginning of the 11th century’ (p. x.)*

The years in the *Annales* are written down to 977 though the last event recorded is in 954, which points to their being finished c. 954 or 955, according to Phillimore.

‘The chronology of this document’, says Ab Ithel ‘is designated by the repetition of the word “annus” for each successive year, whether blank or otherwise, whilst every tenth year is marked x, xx, etc. . . . From a comparison of the dates assigned to many of the events noticed in it by other writers, it would appear that the era on which its chronology rests would concur with the year’ [445] ‘of the Incarnation, though there is no reason given for this particular date’. (p. xxiv). He has compiled his marginal dating of the entries by adding an extra year for each annus mentioned, from 445 [=year 1] onwards, and correlating with Christian dating. A glance at the facsimile from the manuscript which is given as a frontispiece to the volume, shows at once how the dating has been arrived at.

I can see no obvious flaw if known dates such as the death of Offa in 796 (‘Annus CCLII’ plus the previous 444 years of the Christian era) be taken as test cases. On the other hand the two dates 537 and 547 do not, as the following argument shows, seem to be correct for the outbreaks of pestilence they chronicle.

Justinian’s plague was raging in Constantinople in 542, and appears to have spread north-westwards across Europe. We know from Gregory of Tours (*History of the Franks*, iv, 5) that various parts of France were suffering from it in 543 : the plague, called *blefed*, reached Ireland, according to the *Annals of Ulster*, in 544 (really in 545, since these annals are one year behind the true date at this period). If it was carried to these islands from Armorica by means of Celtic saints, it is possible that it may have arrived in Wales and spread to Ireland or the opposite may have happened. There is no record of either pestilence having spread from Wales into England also. The *blefed* must be distinguished from the Yellow Plague which followed it, according to the *Annals of Ulster*, in 548 (really in 549) and which doubtless overlapped it.

* Early 12th century, according to Phillimore.

NOTES AND NEWS

The following table shows the sequence of the outbreaks of the two pestilences, with the recrudescences of the Yellow Plague:—

DATE	PESTILENCE	SOURCE OF INFORMATION
[537.	'Mortalitas' in Britain and Ireland	<i>Annales Cambriae</i>].
545.	'Blefed'. ('Mortalitas prima quae dicitur blefed', <i>Annals of Ulster</i> : 'Mortalitas magna', <i>Annals of Tigernach</i>).	<i>Annals of Ulster</i> (under 544), <i>Annals of the Four Masters</i> (under 543), <i>Annals of Tigernach</i> (539 or 540), <i>Chronicon Scotorum</i> (under 541), <i>Annals of Clonmacnoise</i> (under 546).
[547.	'Mortalitas magna'.	<i>Annales Cambriae</i>].
549.	'Mortalitas magna' in Ireland (the Yellow Plague, <i>cron chonail</i>).	<i>Annals of Ulster</i> and <i>Annals of the Four Masters</i> .
550.	'Boy Connell' begins in Ireland.	<i>Annals of Clonmacnoise</i> .
551.	'Mortalitas magna, i.e. Crom Conaill'.	<i>Annals of Tigernach</i> , <i>Chronicon Scotorum</i> , <i>Annals of Inisfallen</i> (under 541).
556.	'Magna mortalitas'.	<i>Annals of Ulster</i> (under 555).

Mr Johnstone says, 'No other pestilence is mentioned between 500 and 539. The Blefed must be the pestilence mentioned by the *Annales Cambriae* along with Camlann', but as I have just shown, both of the *Annales Cambriae* entries (included in the above table) are earlier than the corresponding Irish entries in the *Annals of Ulster*—the 537 one by seven (or eight) years and the 547 one by one year (or two years). W. BONSER.

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PESTILENCES IN SIXTH CENTURY BRITAIN

Mr P. K. Johnstone's note in *ANTIQUITY*, xxiv (1950), 44 is valuable not only for the light it throws on the date of Camlann, but for the distinction between two pestilences afflicting Britain in the mid- 6th century A.D. Such plagues can be of considerable chronological importance, and, in the 5th century, Mr C. E. Stevens has drawn attention to the probable equation of the *pestifera lues* or *famosa pestis* placed by Gildas shortly before the Saxon invasions with the *pestilentia quae fere in toto orbe diffusa est* recorded by Hydatius in relation to a comet datable to 442 (1). Incidentally, it appears that this plague is independently referred to in the *Vita Sancti Winwaloei*, written by Wrdisten, Abbot of Landevennec, in the second half of the 9th century, (as that which drove Fracanus, cousin of Catovius the ruler of *Nomnia* in Britain (emended by Doble to *Domnonia*) to Brittany some time in the 5th century (2), though Wrdisten may have simply elaborated his story from the mention of plague in Gildas.

¹ *Eng. Hist. Review*, LVI (1941), 363.

² A. de la Borderie, *Cartulaire de l'Abbaye de Landevennec* (Rennes, 1888); G. H. Doble, *Saint Winwaloe* (Cornish Saints, no. 4; 2nd Edn., 1940); Kenney, *Sources of Early Irish History*, I, p. 175.

The recent discussion of the medical aspects of the pre-Conquest plagues of Britain by Dr Bonser and Lt.-Gen. Sir William MacArthur (3) gives point to Mr Johnstone's separation of the 'blefed' of 539-540 from the 'Yellow Plague' of c. 549, the most famous victim of which was Maelgwn Gwynedd. Sir William points out that the explicit descriptions of the symptoms of the *flava pestis* preclude one from identifying it with bubonic plague, and he concludes that it was in fact a severe form of relapsing fever with the common accompaniment of jaundice, probably brought on by a period of famine. This renders connection of this pestilence with the outbreak of bubonic plague in the Eastern Roman Empire in Justinian's time (4) impossible, but the 'blefed' of c. 539 might be related to this, and to that recorded in Europe in 542—*horum exordia malorum generalis orbis terrarum mortalitas sequitur et inguinum percussione melior pars populorum* (5). In the early Irish Saints' lives, however, such as those of Declan, Rua, Mochua, and Finnian of Clonard, the 6th century plague is always described by some phrase indicating it to be the *flava pestis* and there seems no certain remembrance of any other (6). If Sir William MacArthur is correct in connecting this outbreak with a period of famine, it is possible that the story of the famine in Cornwall, in the episode of the visit of the Alexandrian ship to Britain in the 6th century, contained in the Life of John the Almsgiver (died 616), may have a foundation in fact (7). If bubonic plague did indeed reach these islands in c. 539 it is in the context of the trade contacts with the Mediterranean implied by such stories, and by the actual archaeological evidence (8), that its appearance can best be understood.

There is one side issue with regard to the death of Maelgwn in the Yellow Plague of c. 549 that is worth following. The earliest MS of the *Annales Cambriae* (Harley 3859 compiled in the late 10th century) simply records his death by *mortalitas magna*, but we can trace the elaboration of the story in successive centuries in an interesting manner. The *Vita Sancti Teiliavi* in the Book of Llandaff, compiled c. 1150, takes the fact of Maelgwn's death from the *Annales* but describes the *flava pestis* and its accompanying portents in detail (9), but early in the 13th century Welsh tradition in the form of the earliest MS of the Brut describes further how Maelgwn 'went into a church hard by his own castle at Deganwy and it was there he died (10)'. A later version of the *Annales*, of c. 1286, inserts another fragment of tradition after the entry of Maelgwn's death by *mortalitas magna*—*Unde dicitur Hir hun Wailgun en llis Ros. Tunc fuit llalwelen* (11). This seems to quote a proverbial saying 'Long the sleep of Maelgwn in the court of Rhos' and 'lallwelen' is presumably a corruption of 'y fad felen', the Yellow Plague. By the 15th century however this death in the church of Rhos near Deganwy is presented in

³ *Journ. Brit. Arch. Ass.*, 3rd s. IX (1944), 48-71.

⁴ Cf. Procopius, II, XXII.

⁵ Chronicle of Victor Tonnennensis, ed. Mommsen in *MGH Chron. Min.* II (1893), 201.

⁶ Plummer, *Vitae Sancti. Hib.* (1910), II, 48, 242; Whitley Stokes in *Anec. Oxon.*, 1890, 229, 287 (Book of Lismore).

⁷ Leontius, *Vita Scti. Joannis Eleemosynarii*; Migne, *Pat. Graec.*, XCIII, 1614 ff. Cf. V. c.H. Cornwall, Part 5 (1924), 19.

⁸ Cf. Radford in *Trans. Devon Assoc.*, LXXIX (1947), 27.

⁹ Book of Llan Dav (ed. Gwenogvryn Evans, 1893), 107; cf. Doble, *Saint Teilo* (Lampeter, 1942) and in *Journ. Theo. Studies*, XLIII (1942), 204; XLIV (1943), 59.

¹⁰ Dingestow Court Brut (Nat. Library Wales MS. 5266), f. 288.

¹¹ MS. 'B' in Buffus Hardy's edition in *Mon. Hist. Brit.* and in Rolls Edition (1860), written on fly-leaves of an abridged Domesday in the P.R.O.

more dramatic form, describing how Maelgwn, having taken sanctuary, 'saw the Yellow Plague through a hole in the door of the church' (12). It is at this stage that the story is taken up in its well-known prophetic form in the spurious *Hanes Taliesin* (13), where the bard describes the monster with yellow eyes and teeth and hair which will come upon Maelgwn from the marshes of Rhianedd—a legend first given currency in English by Thomas Love Peacock's rendering of the 'Dymma Fustl y Beirdd' in his *Misfortunes of Elphin* published in 1829, a decade before its appearance in Lady Charlotte Guest's translation of the *Mabinogion*.
STUART PIGGOTT.

ANCIENT MINING AND METALLURGY

Shortly after the end of the late war, the Royal Anthropological Institute formed a committee, with representatives of various branches of science concerned, to investigate various problems of ancient mining and metallurgy, and this Committee is at the service of archaeologists and others who wish for assistance upon such problems. It is also very willing to advise excavators concerning the technology of metal tools and other artifacts, and in certain cases to carry out complete analyses of such material.

One of the most crucial problems in study of the development of technology and applied science is the extent to which native copper was used in prehistoric times, and how far the discoveries of its properties of malleability and fusibility preceded that of the art of extracting copper from its ores. It is generally admitted that native copper was used before smelted copper. Indeed, one school holds that early metal-using cultures were dependent upon native copper for a considerable time, so that there would be two phases in intelligent metallurgy (apart from a supposedly still earlier phase in which, as in pre-Columbian North America, copper was worked cold as a superior kind of stone). We badly need data to determine how far cultures using only native copper preceded those using the smelted copper, but this is bound up with a further problem, namely the best method of distinguishing the native copper from the metal from oxidised ore.

These questions are not easy to answer and as it seemed most necessary to clear up such an unsatisfactory position, the Committee started to investigate the problem and has issued a preliminary report (see *Man*, 1948, 3 and 17). In a measure this report cleared the ground by stating the difficulties to be overcome. In order to make further progress, a large body of material must be examined, and therefore archaeologists are asked to advise the Secretary of the Committee (Miss S. Benton, F.S.A., c/o Royal Anthropological Institute, 21 Bedford Square, London, W.C.1) of material from early cultures of which they have knowledge, or which they could send for examination and report.

AMERICAN EXCAVATIONS

The University Museum of the University of Pennsylvania is currently engaged in archaeological research in Iraq, Iran, Turkey, Cyprus, British Honduras and Alaska. All of these researches have begun since 1947 with the return to normal after World War II.

In Cyprus, at the ancient site of Kourion, the Museum is continuing excavations begun several years before the last war. At the moment Dr B. F. Hill, George McFadden

¹² Brut in Cotton Cleo B. V. I am indebted to Prof. Henry Lewis for this information.

¹³ Text in *Myerian Archaeology* (2nd edn., 1870), 29; cf. Ward, *Cat. Romances . . . in Brit Museum I* (1883), 421 for the MSS.

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and DeCoursey Fales are completing the Roman Theatre and the Roman Baths on what is known as Kourion Bluff, also the Sanctuary of Apollo which has produced a remarkable collection of terra cotta figurines dating from several periods in the history of Kourion. Dr and Mrs John Young are preparing the final study of these figurines for publication by the Museum this year. Further publications completing the work at Kourion, particularly those covering the Bronze Age site at Bamboula near Kourion, should be completed within the next two or three years.

In Iraq the Museum has joined with the Oriental Institute of Chicago to continue excavations at the site of Nippur where some sixty years ago the University Museum began its work in the Near East. This is the Mecca of the Babylonians which has produced the majority of the literary tablets or what has been described as the world's earliest known literature. This season of research under the direction of Dr Thorkild Jacobsen and Dr Donald McCown has meant the completion of the Temple of Enlil dating from Ur III and discovery of additional literary tablets which give us fresh knowledge about the religion, mythology and philosophy of the Sumerians. Two of these reported by the Press describe a curious murder trial about 1900 B.C. and something of the moral and religious philosophy of the same period in a tablet concerning the goddess Nanshe. Excavations at Nippur will continue in 1951.

In Iran Dr Carleton Coon has discovered the first extensive remains of an Upper Palaeolithic culture as well as nicely stratified Mesolithic and Neolithic cultures in cave deposits known as the Belt and the Bisutun caves. The large collections representing Upper Palaeolithic, Mesolithic and Neolithic periods are now in process of study but it is possible to say that they indicate some startling conclusions. The Museum intends to continue excavations in Persia also in 1951.

The site at Gordion, some 60 miles from Ankara in Turkey, was begun by Dr Rodney Young and his associates from the University Museum in April of this year. With luck which comes to archaeologists only once in a long time they struck an exciting tomb containing a hoard of gold objects on the first day of excavation. It is a Phrygian tomb which probably dates from the 7th century B.C. and encourages the excavators to believe that Gordion is a most productive site. The excavation of additional tombs and the big city site of Gordion will continue through May, June and July of this year.

Dr Linton Satterthwaite has just cabled the Museum from British Honduras that he has discovered some new and remarkable Old Empire Maya monuments at the ancient Maya city of Caracol. He is laying plans to continue work at this site and at Cayo x where he will dig some house mounds next year. The Museum plans to continue in British Honduras with most hearty cooperation from the Department of Antiquities. To British readers it may not be surprising that other Middle American countries are not at present too cooperative in this kind of research.

In Alaska the University Museum is working in collaboration with the Danish National Museum and the University of Alaska with the joint research under the direction of Dr Froelich Rainey, Dr Helge Larsen, Denmark, and Dr Louis Giddings, University of Alaska. Last year Giddings and Larsen discovered on Seward Peninsula ancient flint industries which appear to link Upper Palaeolithic industries of the Old World with the so-called Folsom and Yuma complexes of the New World and thus give us the first real evidence of ancient connections between the two continental land masses.

The specific researches referred to above indicate the general fields in which the University Museum expects to concentrate during the next several years and reflect the intention to carry on in areas where the Museum has concentrated during the past half century.

FROELICH RAINEY.

Reviews

TABULA IMPERII ROMANI. BLATT M 32 MAINZ. Edited by PETER GOESSLER
Römisch-Germanische Kommission, Frankfurt-Main, 1940.

About fourteen years ago, when the founder of ANTIQUITY had succeeded in collecting together a body of international historians, archaeologists and geographers, to discuss the furtherance of an international Map of the Roman Empire on the scale of 1 : 1,000,000 I had the good fortune to attend the Conference and listen to its deliberations. Apart from the energy and tact of the Secretary, Mr O. G. S. Crawford, what were chiefly noticeable were the determination of some countries who possessed somewhat small portions of the former soil of the Roman Empire not to be left out of it, and the elaborate and detailed scheme that resulted from the discussions. It was a bold venture, and fortune does not always favour the brave, yet in the four years that remained before the outbreak of World-War II definite advances were achieved. This country has to its credit two out of three sections—O. 30 (Aberdeen) and N. 30 (Edinburgh); France one, L. 31 (Lyon), and Italy four,* L. 32 (Milan), K. 32 (Florence), K. 33 (Rome), and J. 33 (Palermo), while Egypt too has produced her whole quota, H. 35 and 36, G. 36 and F. 36. In 1940 Germany added one, M 32 (the name of which had been changed from München to Mainz), and to the best of my knowledge that is the complete total so far. Twelve sheets in five years is not too poor a rate, and we can only hope that with the return of peace (if that is the correct name for the present world-situation) the production of the remaining sheets may begin again, and a reasonable rate of progress be maintained.

This section, edited by Peter Goessler, comprises not only the Map itself, but also in a separate brochure an excellent short Introduction; after this comes a Gazetteer of geographical and political names, and an Index of sites marked on the map (both those which have place-names, and those marked by symbols), and finally co-ordinates for the symbols. The map itself includes (as the Editor says) most of the Roman provinces of *Germania Superior* and *Germania Inferior*, with not inconsiderable portions of *Belgica* and *Raetia*. Its western border runs just West of Aix (Aachen) and Metz, the eastern by Kehlheim on the Danube. In spite of a large mass of detailed information incorporated upon it, the sheet as a whole remains beautifully clear to read and study, and the editor deserves congratulation upon his success.

The map is divided almost diagonally into Roman and non-Roman territory. Far beyond the frontier and to the North-East of the Rhine stand out three Roman legionary camps, Haltern, Oberaden, and Kneblinghausen. These three camps are all that have been identified, up to the present moment, of what must have been hundreds of marching-camps thrown up by legionaries campaigning at various periods during the reign of Augustus; is it not possible that air-photography will reveal some more, and so help to elucidate the history of the Roman invasions of trans-Rhenane Germany, and even possibly (though this may be too sanguine) reveal finally the last camp of Varus? One season of intensive photographing, carefully planned and carried out, could reveal an immense amount of fresh material, as anyone who has glanced at the recent photographs

* The Editor informs me that though these sheets were printed he does not think they were ever published.

of Dr St. Joseph—disclosing sometimes not only new and hitherto unsuspected sites, but further additions to sites already known—could testify.

I do not possess sufficient topographical knowledge of the region which this map covers for me to put forward detailed criticisms, but several points are bound to strike anyone studying it. We can observe how rich and thorough was the Romanization of the Palatinate and Eastern Lorraine; the map indicates a quantity of ruins and remains of buildings unidentified, and many villas and temples; the stone was quarried in abundance, there are many symbols denoting mines and potteries. For the density of habitation remains the region is worth comparing with Central Southern England in Roman times. Secondly, what stands out on the map is the amazing line of the long aqueduct which provided Cologne with water, and one may remark also that the Romans spared no expense in providing aqueducts even for comparatively small towns, as e.g. Rottenburg. Thirdly, we note the extent to which the Roman names for places have still survived in a fairly recognizable form, suggesting the probable survival of much of the older population during the period of invasions. And finally, glancing again at the map as a whole, we must congratulate editor and printers alike upon the exceptional clarity of it, and upon the reliability of the Gazetteer and of the indices.

One incidental note: those who are interested in adding to the picture here presented will find it well worth while turning to the maps and plans in the excellent article by Mrs Brogan, 'Trade between the Roman Empire and the free Germans' (*J.R.S.*, xxvi, 1936, p. 195), and from these plotting out roughly upon the Mainz sheet the principal finds of Roman coins and objects in trans-Rhenane Germany; one thick concentration that seems to me noticeable is that around the waters of the middle and upper Saale upon routes from Mainz itself and from Paderborn.

In conclusion let me express a hope. Is it too much to expect that we may yet see sheets M. 30 (South England and North Brittany) and L. 30 (Western France) completed? It may be that this cannot be carried out under the international system formerly planned, but might it not be done by scholars of the two nations, upon the agreed scale and under the recognized symbols? Similarly for the Dutch and Belgian sections: we know that our Dutch colleagues have been doing notable work along the lower Rhine, and our Belgian colleagues upon the problem of the late Roman frontier—to mention two aspects only; if something equivalent to sheets M. 31 and N. 31 could be produced, what an advance that would mark. To some extent on sheet N. 31 (Amsterdam) additional symbols might have to be employed, so as to indicate roughly the main finds north of the frontier in barbarian territory, but that should not be difficult. The completion of this whole section, comprising central Western Europe, would prove a work of immense value for classical and historical students alike. Cannot UNESCO be persuaded to take an interest in the project and to further it?

M. P. CHARLESWORTH.

THE EXCAVATION OF STE MARIE I. By KENNETH E. KIDD. *Toronto University Press*, 1949. (London: Geoffrey Cumberlege), 191 pages, with plates, maps, plans and line drawings. Price 26s.

Ste Marie I was for ten years the headquarters of Jesuit missions to the Huron Indians. It was at once a fortress, a retreat for priests, and a centre for the diffusion of Christianity and European customs. Building operations were begun in 1639, and the site was abandoned in 1649. In 1648–49 the Iroquois attacked the Hurons, conquered their territory and destroyed their power. Ste Marie itself was not attacked, but the Jesuits fired it on 15 May 1649 and followed the surviving Hurons to their temporary

refuge on Christian Island. Here they began to build a second 'residence', Ste Marie II, but in 1650 this too was abandoned, and Jesuit missionary activity among the Hurons came to an end.

In 1940 the Jesuits bought the site of Ste Marie I, and in 1941 Kenneth E. Kidd's excavation party was in the field. The present volume is the excavation report. It is more important and it will have a wider appeal than most excavation reports, for it will attract and inspire many scholars who have no special interest in either Jesuit missions or Canadian history. It is a landmark in archaeological and historical studies. It shows what can be done when archaeological methods are directed against a comparatively modern site, against a site for which, as for Ste Marie I, there exists an abundance of historical evidence. The short and tragic history of Ste Marie I is known in considerable detail, but there are many gaps. Only the archaeologist and the spade could provide information on the size and nature of the buildings, on the methods used in their construction, and on the general culture (i.e. ornaments, pottery, tools and general equipment) of Europeans and Indians in this area.

In Britain archaeologists seldom venture far into the historical period, though there are many sites, deserted villages, derelict chapels, medieval farm buildings and the like, that are crying out for attention. Scholars like C. W. Phillips in England and W. Douglas Simpson in Scotland are leading or driving archaeologists further into the historical period, and we may soon accept without surprise the excavation of even a 19th century site. Archaeology can make as great a contribution to the history of the Industrial Revolution as to the history of the Anglo-Saxon Settlement. Canada has set an example which could be followed with profit in Britain.

The excavation of Ste Marie I is important, of course, for its own sake. It has settled many problems, and it has provided much new information on a spectacular episode in Canadian history. One might have expected excavation to reveal more clearly the limits of the largest building on the site. One might have hoped for more evidence about the superstructures. And, in Europe at least, one might raise the traditional cry for more sections. But it would be as unwise as it would be unfair to criticize, even obliquely, a pioneer excavation carried out under conditions quite unfamiliar to us. The thoroughness of the destruction in 1649, rather than any lack of excavational skill, should perhaps be held responsible for the gaps that still remain in the story of Ste Marie I. Mr Kidd's operations seem to have been characterized at every stage by care, foresight and scientific precision.

The arrangement of the report is admirable. The historical background is briefly sketched, and there follows an account of the excavation, a long section on finds, a short summary, three interesting appendices and a useful bibliography. Extremely valuable is the author's explanation of how the initial survey and the excavation itself were planned and executed. The methods as well as the results are laid before us, and for this we cannot be too grateful.

F. T. WAINWRIGHT.

A CONCISE ECONOMIC HISTORY OF BRITAIN FROM THE EARLIEST TIMES TO A.D. 1750. By SIR JOHN CLAPHAM. *C.U.P.*, 1949. (xvii and 324 pp.) 12s 6d.

On the dust jacket of this book appears a Cambridge engraving of 1690. In the foreground the open fields are being reaped and in the background (perhaps a symbol) ? stands Sir John's College and his University. The engraver must have stood very near the present site of the Library where the Professor of Economic History worked.

This book will be widely read and widely used ; the dust jacket will perish, and the publishers have not reprinted it (or any maps) as endpapers. Perhaps they assume that the reader will by then have assimilated Cambridge and not require the visual aid ! For the book represents not only what the author himself taught, re-cast in book form at the end of a long teaching life, but what additions to economic history have been made by those whom he taught and those whom they taught. It puts down on paper a tradition which has been largely oral, and its unconventionalities can be seen by comparing this book with the other *Economic History* in the field, Lipson's, written in 1915 and revised in 1937 and 1946.

The striking characteristic of this book is its ability to move in both time and space : places as well as people are actors, and the author is a true traveller. The essential variations in regional life, in the non-typical manor, are well brought out in pp. 86-93 ; probably more clearly to the reader than the account on pp. 110-24 of when and how the 'typical' manor disintegrated. Has the question recently posed by Dr R. H. Hilton (1) really been answered : what is there in our description of the typical manor to tell us why it should not have gone on indefinitely without change ?

In the origin of the Saxon open fields Sir John plumps for the Scottish analogy : the open fields come from the earlier in-field. This reviewer has helped to put some in-field back on to the medieval English map in certain geographical conditions, but he is reluctant to call in this intermediate stage to explain what, to him, Dr C. S. Orwin has satisfactorily explained : given men on the defensive ; given rare and expensive capital equipment like the plough ; given a social structure worked out in the forests of north western Europe before the invasions ; given the great pull on man and woman power in the first clearing . . . given these, the furlongs and strips and scattered holdings of the nucleated open field village arise in the very acts of colonising the waste.

With characteristic candour and caution the author puts down his view of the pre-historic and Roman fields : this reviewer can do nothing but agree.

Mr John Saltmarsh has prepared the MS for the press with an admirably full contents summary, page headings and cross references. There is no book-list, and some lack of uniformity in whose work shall have footnote allusion and whose not. Mr T. A. M. Bishop whose work is clearly the basis of some sentences gets no footnote, but a short phrase from Mr Hewitt's *Medieval Cheshire* does. Perhaps the reason is that Cambridge might be expected to have read Mr Bishop but not to be as appreciative of Cheshire work as Sir John (and this reviewer). This reviewer also derives some satisfaction from having helped to dig his way literally into a footnote on page 80. It is something of an achievement to have caught Homer nodding but Mr Saltmarsh confesses that Sir John overlooked the quantity of abandoned medieval village sites whose rarity he more than once affirmed (2). As long ago as 1936 Clapham's colleague H. C. Darby had written of Canon Foster's pioneer listing of the many Lincs. sites (3), and W. G. Hoskins' Leicestershire article (4) may have seen daylight before this book was completed. It is a curious oversight in a man whose whole book demonstrates that the good historian keeps his eyes wide open in his space travels as much as in his time travels. Readers will find this

¹ *Studies in Leics. Agrarian History*, ed. W. G. Hoskins, 1949, p. 19.

² Indeed one even provides a windmill, earlier than his earliest example. It was at Weedley [E.R.] in 1185 : see B. A. Lees, *Records of the Templars*, p. 131 [London, 1935.]

³ H. C. Darby, ed., *Historical Geography*, 1936, p. 208.

⁴ *Trans. Leics. Arch. Soc.*, xxii, 242.

book a good travelling companion, and the Press have published it at an almost pre-war price-level. It is to be regretted that those who borrow from libraries will probably never see the dust jacket, since most libraries expose them at birth. Perhaps the next edition will see some illustrations in the text: we still badly need an accessible book with part of an open field map actually reproduced and not sketched or transcribed.

M. W. BERESFORD.

EARLY KHARTOUM. An account of the excavation of an early occupation site carried out by the Sudan Government Antiquities Service in 1944-5. By A. J. ARKELL. *Oxford University Press, London, 1949.* XVI+145 pages, 12 text figures, 113 plates. 12 in. \times 9½ in. Price 5 guineas.

This handsome if rather expensive volume is noteworthy as being the report of the first archaeological excavation financed, from inception to publication, by the Sudan Government itself. It is primarily an account of the excavation—or partial excavation—of a mound on the southern outskirts of Khartoum, but, says the author in his preface, it 'is not the mere record of an excavation . . . but a work which will have to be taken into consideration by prehistorians in north Africa and elsewhere'. This is a justifiable claim.

Those interested in the archaeology of the Sudan have for long had legitimate cause for complaint that some of the excavations carried out in that country in the past have been far from adequately published. Mr Arkell has seen to it that no reproach on the score of inadequacy should be levelled at his own publication, for in this record of a comparatively modest excavation there are no fewer than 113 large plates, some in line but most of them in half-tone. Even potsherds are illustrated by full size photographs, and excellent photographs they are.

A preliminary account of this excavation has already been published in this journal.⁽¹⁾ It is sufficient here to say that the low mound which was the site of the excavation was one of the two main cemeteries of Khartoum during the siege of the city in 1884-5, and it was covered with Mohammedan graves. But there was also surface evidence which suggested a very much earlier history, and excavation yielded barbed bone spear-heads, flaked implements of quartz and rhyolite, hammer stones, grinding and rubbing stones, and potsherds of a type not previously published. There were also animal bones, agglomerations of fish bones, and the fossilized remains of several early human burials, as well as some of the Meroitic and other periods. Most of the animal bones were those of animals still common in the southern Sudan, but amongst them, surprisingly, was part of the skull of an extinct and previously unknown reed rat, *Thryonomys arkelli*, related to forms found in deposits in the northern and western Sahara.

From all this Mr Arkell has built up a picture of a community of primitive negroid hunting and food-gathering people who apparently had no domesticated animals, but who had well-made red-brown pottery decorated with wavy lines combed into the soft clay. He shows that the implements used to produce this decoration were the protective spines of a type of cat-fish which still abounds in the Blue Nile. From the evidence available he concludes not only that the people of the early settlement exhibited a mesolithic culture but that the settlement itself was of mesolithic date. He finds at least three early cultures represented on his site and he traces out their connexions with comparable cultures in Egypt and other parts of north Africa. He illustrates these by a

¹ ANTIQUITY, No. 84. December, 1947.

map showing the probable lines of communication between Early Khartoum and sites in the southern Sahara and the Tunis region.

The characteristic culture of Early Khartoum was the Wavy Line culture, that of the people who made the pottery decorated with wavy lines, and this is regarded as being ancestral to the Badarian in Egypt. The bone spears from Early Khartoum are also considered to be earlier in date than the earliest known in Egypt. Somewhat later than the Wavy Line culture came the Gouge culture, represented on the site by only a few sherds; still later, and possibly of proto-dynastic date, came the Omdurman Bridge culture, this last deriving its name from the pottery found in two graves discovered near the bridge connecting Omdurman with Khartoum. The Wavy Line culture was not confined to the Nile Valley but was spread more or less across the Sudan in the latitude of Khartoum. Mr Arkell gives a list and a distribution map of sites in the Sudan on which Wavy Line pottery has been found and he concludes: 'It may therefore one day be established that pottery of this type, perhaps invented in Asia, was spread by negroid people right across Africa through what is now the (northern) Sudan and the southern Sahara, before it entered the lower Nile Valley to provide one of the features of the early civilization of Egypt'.

There is a fine imaginative sweep in all this. Here Mr Arkell displays his gift for relating the particular to the general and, whether or not his highly original and interesting conclusions are ultimately confirmed, he has made a notable contribution to north African pre-history.

Against this wide background the actual history of the Khartoum mound may appear unimportant. Yet one of the objects of an archaeological excavation is to reconstruct as fully as possible the history of the chosen site, and there are certain aspects of the history of Early Khartoum which might have been more fully explained.

We are told that the early settlement was small in area and was situated on the top of a sandbank on the edge of a Blue Nile which then rose in flood about four metres higher than it does to-day. Since the time of the early settlement about two metres have been eroded from the top of the sandbank resulting in a marked flattening of the topography and the burial of what was the Blue Nile flood plain at the time of the settlement. (A section illustrating this was published in *ANTIQUITY* (2) and so does not appear in the book). During this period in the history of the mound precipitation of calcium carbonate (kankar) had formed, due to the fluctuations of the water-table, and its presence had resulted in the fossilization of the human bones and the calcification of the objects belonging to the early settlement. The deposition of kankar had ceased before Meroitic times so that the bones in the graves of this period are not fossilized. Also, a good deal of evidence is provided to show that, when the site was first occupied, the rainfall in the latitude of Khartoum was much heavier than it is to-day.

Since no stratification could be detected on the site the human burials form the only chronological sequence that can be observed. It is admittedly imperfect because graves vary in depth and it does not necessarily follow that bodies found at the same level have been buried from the same surface. With these reservations it is to be expected that, in a mound which has been subjected to erosion, the burials of different periods ought to appear at progressively lower levels. In the report before us the burials are dealt with in separate groups, and the squares in which they were found are indicated in separate text figures. But this treatment, however convenient on archaeological grounds, does not give a complete picture. All the graves were in the same mound and were equally part

² *loc cit.*

of its history, and if their relationship to each other is to be properly understood as many of them as possible should have been shown on the same vertical projection. If this is done it will be seen that the Meroitic burials lie in two groups at different levels, and it is not surprising to find that Mr Arkell, for purely archaeological reasons, considers the group at the higher level to be the earlier in date. The two are at such a distance apart as to suggest that rapid erosion of the mound occurred during the interval of time by which they are separated. It will be seen, too, that the Meroitic burials on top of the mound—the area which has suffered most erosion—are at levels which differ little from those of some of the burials of the early settlement in near-by squares. This is a curious circumstance which can be interpreted in various ways. In any case it would seem that the erosion of the mound was neither continuous nor constant in rate. These facts, possibly, concern only the history of the mound, but they are relevant to the wider issues in so far as the date of the early settlement is concerned. As to this, Mr Arkell would have strengthened his claim to a mesolithic date if he had considered these points and reconciled them with the deductions he has drawn from a study of his finds.

There are some interesting parallels between the objects from Early Khartoum and those from the much more extensive excavations of the late Sir Henry Wellcome at Jebel Moya (3), about 150 miles south of Khartoum, which had not been published when Mr Arkell wrote. Dr Derry, in his report on the human remains from Khartoum, says, of the only skull from that site which could be reconstructed, that its description exactly fitted that of the skulls from Jebel Moya which he examined in 1911–12. The only difference was that the Khartoum skull lacked the upper incisors, whereas at Jebel Moya the lower incisors were sometimes removed. Dr Derry also observed that the bones from Khartoum were so impregnated with calcium carbonate as to ring like pottery when struck. So also were many of the bones from the (nearly 3000) graves at Jebel Moya, although they were not completely fossilized. Yet the skulls from this site which Dr Derry examined came from a cemetery in which some of the graves contained Napatan (*ca.* 750–550 B.C.) beads and copper hair ornaments, sometimes *in situ* on the body. At Jebel Moya, too, there were flaked stone implements, chiefly of quartz, of much the same kind as those from the Khartoum site, also hundreds of grinding stones similar to those from Khartoum, and quantities of stone rings, mostly broken. All these types of stone objects occurred in the same strata as Napatan amulets and datable scarabs, and Jebel Moya was a well stratified site. On the lower levels there were potsherds not dissimilar, to judge only from photographs, to some of those from Khartoum, and the fragments from Jebel Moya are not likely to be of a date much earlier than 1000 B.C. The sherds from Taferjit and Tamaya Mellet in the French Sudan which Mr Arkell illustrates on Plate 102 appear—again judging only from photographs—to have closer affinities with the early pottery from Jebel Moya than with that from Early Khartoum.

The only inferences which can safely be drawn from these facts are that certain types of flaked implements had a range in time of several thousand years, that some kinds of pottery in the Sudan had a long history and a wide distribution, and that human bones can acquire a considerable degree of fossilization in less than 3000 years. They lead to nowhere in particular, unless they point to the need for some method, which in most cases does not exist, of cross-checking the dates of ancient sites by reference to criteria which are not purely archaeological.

In spite of all this, it may well be that Mr Arkell's reconstruction of the movements of early peoples is close to what actually occurred. His book is a considerable achievement,

³ *The Wellcome Excavations in the Sudan*, parts I and II. *Jebel Moya*, by F. Addison.

and his detailed publication of his finds will provide archaeologists and prehistorians with much new material for study and a good deal to think about. It is to be hoped that the Sudan Government, having once committed itself to the publication of full archaeological reports, will not draw back and that Mr Arkell's next volume will not be long in appearing.

F. ADDISON.

CAMBRIDGE EXCAVATIONS IN MINORCA. By MARGARET MURRAY, D.LITT., F.S.A. (Scot.), F.R.A.I. *London, B. Quaritch.* TRAPUCÓ, Part I, 1932. SA TORRETA, 1934. TRAPUCÓ, Part II, 1938. 12s 6d each volume. (The last named has only just been published).

For the purposes of this review the reports are numbered I, II and III. They cover three season's work on two sites near Mahon in Minorca, one, Trapucó, a mile south of the town, the other $5\frac{1}{2}$ miles to the north of it.

Two enclosures each containing a 'taula' were cleared, one at either site; also the remains of what may have been a 'naveta' at Trapucó; and irregular chambers at both sites.

It was to be hoped that these excavations might have solved the problems of the date and purpose of the constructions known as 'taulas', which are found in that island, and probably existed in Majorca. They occur in association with the strong circular towers known as 'talayots', which closely resemble similar buildings in Sardinia termed 'nuraghi', which were the homes of local chieftains.

The taula consists of a rectangular monolith carrying a flat table stone, the pair giving a T section. The two stones forming this construction are often carefully dressed and stand excentrically within a D shaped area contained by a ring of upright slabs embodied in a dry stone wall; the slabs are set athwart the wall with the long axis in each case pointing towards the taula. The taula at Trapucó was found to rise to a height of 16 feet and the enclosing slabs apparently to some 10 feet.

The obvious explanation of this arrangement is that on the central cap stone rested a series of cross beams which were carried by intervening pillars to the slabs in the outer wall and that these beams supported the roof. Such a construction is frequently found in Minorca and Majorca on a smaller scale—and is illustrated in a cruder form in plates VII and VIII of vol. II.

Miss Murray rejects this explanation however in favour of a more romantic theory that the upper stone of the taula was an object of veneration 'the emblem, the outward and visible sign of the Deity, raised on high to be viewed by all the people'. (She is uncertain of the sex of this deity). She records the remains of corbelling in two of the corners of the enclosure in which stands the Trapucó taula (I, pl. ix, 3). The 'built pilaster' in that illustration, consisting of 5 superimposed rough stones, should, by analogy with other sites, have been plastered over to simulate a monolith, and if similar constructions formed the intermediate roof pillars they might well have vanished leaving no trace for any but careful and expert excavators. Free standing uprights still carrying rough cap stones and supporting a roof are illustrated in Vol. II, plates VII and VIII.

Whatever the ceremonial use of the taula buildings may have been—and the careful finish of the taula itself points to some especial importance—they seem to have formed a 'hall' which was surrounded by a series of lesser chambers, most or all of which were subterranean or without windows. It should be remembered that the earliest accounts of the people of the islands describe them as wearing little clothing, if any, and inhabiting natural or artificial caves.

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Unfortunately we are given no general site plans of the groups of buildings to indicate the relative positions of talayots, taulas and the many subsidiary chambers, nor are there proper or comprehensible plans of the areas excavated; indeed for the site reported upon in Vol. II there is only a sketch plan without scale, as 'the measurements and the plan drawn on the spot have disappeared'.

There are no sections of the ground excavated, although the varying types of the pottery alone indicate long residence and re-use of the sites.

There are however many good photographs and drawings of the very varied assemblage of pottery; but no scales appear on the drawings in Vol. III and for the photographs only occasionally a portion of a rule is included with the fragments of pottery. When they are legible these rules represent either 10ths or 12ths of unspecified units of length.

For the dating of the different types of monument little evidence is offered; but Miss Murray records her opinion, based on comparison of the structural details of the temples of Malta with the building methods of the Minorcan enclosing walls, that the latter 'belong to the first half of the Bronze Age'; as in her view, they cannot be contemporary with the taula, 'it would appear that the taula must have been erected originally in the open and enclosed at a later period'; also that 'the date of the taula is of the beginning of the Bronze Age' (III, p. 36) but 'the nature of the working would show that the taula of Trapucó belongs to the Neolithic period' (I, p. 11).

Miss Murray produces a new theory to account for the form of the tombs of megalithic masonry which were certainly designed to reproduce the rock cut caves with all their elaborate detail. They are locally known as 'navetas' from their superficial resemblance to an up-turned boat; Miss Murray's verdict is 'It is probably a representation, on a large scale and in stone, of an original boat-burial'. (II, p. 22).

The volumes are in fact of no scientific value as records of these Balearic monuments, and their usefulness for students of the pottery, which might have been very great, is reduced to the minimum by the failure to record stratification.

An exception should be made however in favour of the very careful record of human remains by Dr John Cameron in Vols. II and III. And, when scientific excavation of other sites has established the dates of the different classes and forms, these illustrations of pottery and the original material, which is presumably preserved at Cambridge, should be of considerable value.

The cost of the three expeditions was borne by the Cambridge Museum of Ethnology, thanks to the very generous contributions of its Director. W.J.H.

EARLY SCOTLAND. By H. M. CHADWICK. pp. xxix+171. Cambridge University Press, 1949. 15s.

Cambridge paid to Chadwick the honour it accords to its greatest scholars when it made of him a myth. Elusive, as was supposed, to the point of being unapproachable, the student of many languages and of remote branches of learning was traditionally hidden behind his published works. We had learned from the *Heroic Age*, and from the world-wide survey in the *Growth of Literature*, how earlier peoples are to be known through their orally transmitted literature; how tradition and myth may tell us more of a race than does its veridical history; how the entertainment in the hall and the stories related round the fire are a more living part of our heritage from earlier ages than is a knowledge of their politics and their economics. This widening of our approach to the past was Chadwick's share in the revolution in thought wrought half a century ago by the *Golden Bough*.

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In this posthumous work, and in Mrs Chadwick's introduction to it, we are admitted to see a little more of the elusive scholar. It was not for nothing that Chadwick was named Hector Munro. In intervals of his already known studies he was at work on Celtic Scotland and, as the illustrations show, was travelling to obtain that intimate knowledge of the geography of the land of his ancestors without which the peoples and their history cannot be understood. What we have as a result is a series of studies of Pictish and to a lesser extent Dalriadic and British history, which will be prolegomena to all future investigations in these ill-cultivated fields. The wide range of questions condensed into the chapters of this short book is summarized and brought together in Mrs Chadwick's able introduction, which serves at the same time to remind us how far ahead final judgment on so many of these issues still lies. It was Chadwick's ambition to found a school of study of the Late Prehistoric and Dark Ages, and it may be hoped that the new points of departure provided by this book may serve as a challenge to future research by students in all the branches of learning which this catholic scholar was able here to bring into contribution.

For the book is notable for the width of its approach. In the interpretation of the annalistic sources, which was successfully undertaken so long ago by W. F. Skene. Chadwick has fewer, though still notable, contributions to make; and the opportunities for further advance by purely historical methods are plainly limited. His chief contribution is the reinterpretation of the relations of the kingdoms of the Dalriadic Scots and of the Picts which, in the 9th century, brought a Scot to the throne of a combined kingdom. The study of the genealogies, and particularly of those of the South Scotland families which are preserved in Welsh sources, yields a series of inferences of the greatest interest regarding the re-establishment in A.D. 369 of the northern frontier by the creation of Romano-British kingdoms which long survived the withdrawal of the legions. In the field of Celtic linguistics a fresh synthesis is achieved and related to the movements of population, from the Continent to Britain and from Britain to Ireland, from the Late Bronze Age onwards, which are beginning to achieve definition as a result of the work of archaeologists. A study of the distribution of place-name elements breaks new ground, despite the pioneer achievement of the late Prof. Watson, and yields results regarding the distribution of Gaelic and Welsh-Pictish speech at different periods—results which are associated with both population movements and changes in political domination. Finally, and notably, archaeology is made to yield its full contribution to the problems of Scottish history throughout the later first millennium B.C. and the first millennium A.D.

From this too brief survey the scope of the book will be realized. That so much can be contributed in these closely woven chapters is a tribute to the width of the author's scholarship and to the alertness of his sympathetic understanding. But Chadwick would have wished the book to be regarded, not as an end, but as a new beginning, and to none of us is it so much a challenge as to the archaeologist. Faced with the obligation to cooperate with Chadwick as historian, as philologist and as place-name student, we realize how short a distance we have gone towards building up a reliable picture of the cultures, the tribal groupings and the population movements of early Britain. L.S.

BEFORE PHILOSOPHY, by H. and MRS. FRANKFORT, J. A. WILSON and T. JACOBSEN.
pp. 275 *Pelican A* 198, London, 1949. 1s 6d.

Penguin Books deserve congratulations and gratitude for producing this English edition of a work by the same authors published in Chicago under the title 'The Intellectual Adventure of Early Man'; and I do not think the omission of a long section on the Hebrews is to be regretted. The American title was perhaps more accurate; for

what the authors here present is really the oldest extant philosophy—the world view of the first literate peoples, Egyptians, Sumerians and Babylonians, and their conceptions of Nature, Man, Society and the End of Life. Being a preliterate archaeologist, I am incompetent to assess the correctness of the presentation, since I cannot check how far the quotations with which they copiously document their account are accurately translated and representative. But I can say that what they present does fit remarkably well in the general pattern of mankind's intellectual development as it is seen by modern anthropology.

Frankfort evidently agrees with Durkheim : ' Logic presents different characters at different periods of history ; it develops, like societies themselves ; its laws, far from being graven from all eternity on the mental constitutions of men, depend upon factors that are historical and consequently social '. It is the second stage of development that is here described. Let us consider the first.

Our prehistoric ancestors, to judge by preliterate peoples of today, conceived of nature as alive and indeed coterminous with society. If they constructed for themselves a conceptual working model of reality, it would be built on the analogy of society, not indeed because society is the most familiar part of man's environment, but because it is the most predictable and so, within the limits of the customary behaviour pattern, the most amenable to modification. But savages and barbarians have no opportunity for *thinking out* a conceptual model of reality, as against *acting on* its more obtrusive bits and pieces. There is no class exempt, as a class, from the urgent task of wringing a livelihood out of reluctant nature ; even a Maori chief must ' be industrious in collecting food '. There is therefore no divorce of theory from practice. Thinking is expressed in action whether that be the effective activities of craftsmanship or the futilities of magic rituals and spells (of course even these have a functional justification, but not what their practitioners suppose).

The urban revolution evoked for the first time a class of literati ' exempt from all manual tasks ' (Wilson quotes the passage but without indicating its significance). Supported from a social surplus, produced by the peasant masses and concentrated in the granaries of the temple or the pharaoh, these had leisure for speculative thought ; it is the fruits of their leisure that are collected here. Naturally they did not start thinking *in vacuo*. They had inherited from prehistory those ' priceless instruments of human thought ', categories, and objectified thoughts, including the rites to which they owed their privileged position.

So the pattern for their model of reality was still society. ' Any phenomenon ', writes Jacobsen, ' which the Mesopotamian met in the world around him was alive, had its own personality and will. To understand nature, the many and varied phenomena around man, was thus to understand the personalities in these phenomena, to know their characters, the direction of their wills and also the range of their powers . . . Intuitively the Mesopotamian applied to nature the experience he had of his own human society, interpreting it in social terms '. But society had changed since prehistoric times. Now there are rulers. They need not soil their hand by pushing and pulling things. Instead they issue commands—spells that are really effective—and their servants push and pull at their behest. So nature, still animate, becomes filled with personal gods who can likewise issue commands. Their behaviour, like that of earthly rulers, can be modified by bribes and entreaties, though it still needs re-inforcing by magic.

' The emancipation of thought from myth ', representing a third stage in intellectual development and sketched very briefly by Frankfort in the last 14 pages, took place in a different kind of society again. The Ionian philosophers, in contrast to the literati of

the Near East, were not drawn from a class set apart 'to concern themselves with spiritual matters', but engaged in public life and practical activity. Even in the 6th century Greek citizens stood much nearer to tribal barbarism than did a citizen of Memphis in the 26th or of Ur in the 20th; for the Minoan civilization had been overwhelmed by barbarism, its ruling class swamped by Achaeans and Dorians and its religion largely replaced by older or more primitive popular cults. If nature were still alive and even social, it was not effectively populated by equally personal gods (such had retired discreetly to Olympus as Cornford remarked). In so far as the order of nature was modelled on the order of society, the latter was neither that of barbarism, where every man's place is determined by custom, nor of a theocracy where it is fixed by the ruler's will, but of the new commercial polis, where wealth, reckoned numerically in coins, determined status. On the other hand, thanks to the improved tools of the Iron Age, the crafts were able to offer controlled processes as bases for the conceptual model, and the Ionians, as Farrington has insisted in another Pelican, seized this opportunity.

The preceding paragraphs do not pretend to be a summary of the book's contents. Within the two thousand years it covers, 'Before Philosophy' has a much wider range. It is 'a study of the primitive myths, beliefs and speculations of Egypt and Mesopotamia'. That does not mean a rehash of Erman and Langdon, seasoned with newly discovered or freshly interpreted documents. The whole presentation is fresh. The vivid depiction of the Egyptian and Mesopotamian environments and their influence on the two world views is particularly enlightening. Jacobsen's account of the Sumerian temple-state as a primitive democracy ruled by a council of elders and an assembly of all freemen which might appoint a 'king', is quite novel. He has of course stated the arguments in detail in learned journals. The most plausible are based on the pictures of the cosmic State of the gods presented in myths which, if they really reflect the organization of the earthly state, must go back far beyond Urukagina to the 'protoliterate period' (*scil.* Uruk) or even al'Ubad.

V. G. CHILDE.

SEMITIC WRITING: From Pictograph to Alphabet. The Schweich Lectures of the British Academy, 1944. By G. R. DRIVER, M.A., F.B.A. *Oxford University Press*, pp. xvi, 222, 57 plates. 25s net.

The *fons scientiae* overflows the narrow vessel originally destined to contain it, and very few of the scholars who have delivered the long series of Schweich Lectures since their inception in 1908 have contrived to keep their material within the bounds of three short lectures. Professor Driver's generous overflow has only been exceeded by the late Professor S. A. Cook who has beaten him by a short head of 30 pages, but this is offset by a win for Professor Driver in the matter of illustrations, with 57 plates and 97 text figures as against Professor Cook's score of 39 plates and no text figures: so with regard to statistics the honours may be held to be equally divided.

But, jesting apart, we have here not only a generous profusion of material of the greatest interest, but a most skilful and illuminating treatment of the many problems with which this fascinating subject abounds. Considering the immense advances which have been made in our knowledge of the history of early systems of writing during the last half-century, it was high time that the subject should find a place among these lectures; and it was fitting that the son of the eminent scholar who inaugurated the series should be the first to deal with it.

Professor David Diringer's learned book on *The Alphabet*, which appeared shortly before the present volume, has already been reviewed in this journal. While both books deal with the general problem of the origin of the alphabet, Professor Driver has

limited the scope of his treatment to the Semitic field, and hence has been able to deal more intensively with matters immediately relevant to the development of Semitic forms of writing. For example, his first lecture, dealing with the cuneiform scripts, occupies 77 pages, and brings together all the most recent knowledge and all the latest theories concerning the origin of that form of writing, its development, its materials and technique, with full documentation. The only other treatment at all comparable is in the late Professor Chiera's book *They Wrote on Clay* (Camb. Press, 1939), where the subject is dealt with in a much more popular fashion, and far less exhaustively.

Professor Driver is of the opinion, no doubt justified, that economic necessity was the mother of the invention of cuneiform, since the earliest collections of tablets come from temple archives, such as those at Erech, and appear to be wholly concerned with temple accounts and revenues. He also suggests that the same reason led to the invention of the hieroglyphic script among the ancient Egyptians, possibly under Sumerian influence. Not all Egyptologists will be prepared to agree with this view, and if the palette of Narmer is the earliest example of pictographic writing in Egypt it may furnish some ground for questioning this assumption. On the other hand, the well-known pictographic tablet from Kish, reproduced in Plate 1, fig. 2, of Professor Driver's book, shows several signs closely resembling certain Egyptian hieroglyphs; in the case of some of these, such as the human head, hand, foot, and *membrum virile*, the resemblance need not be explained as due to the influence of either script upon the other; but a problem is raised by the sign representing a sledge with a hut or some other object on it. It is known that the sledge remained in use in Egypt throughout the Old and Middle Kingdom periods, but evidence is wanting to prove its use in Babylonia before the introduction of the wheeled cart, at a date prior to the 1st dynasty of Ur; hence the appearance of this sign on the Kish tablet might be taken to suggest Egyptian influence. Professor Sidney Smith's cautious judgment on the point is worth quoting; speaking of the two scripts, i.e., the cuneiform and the hieroglyphic, he says, 'There is no ground for connecting the scripts; it would be equally rash to deny the possibility of a connection'. (*Early History of Assyria*, p. 12).

The second lecture deals with the subject of alphabetic writing, and is prefaced by a full account of early writing materials and implements. Professor Driver has an interesting interpretation of the somewhat obscure passage in Isaiah xxviii, 9-10; the English Revised Version renders v. 10, 'For it is precept upon precept, precept upon precept; line upon line, line upon line; here a little, there a little'; but Professor Driver proposes to translate it, 'For it is s-s, s-s, q-q, q-q, a lad here (and) a lad there', and takes the verse as a description of a schoolmaster teaching scholars the alphabet by calling out consecutive letters and making the boys repeat them after him. He suggests that the words rendered 'precept' and 'line' are really the sounds of the consecutive letter *šadde* and *qoph*, as we might speak of p's and q's. The proposed interpretation is most interesting, and removes what has always been recognized as a difficulty, the forced meaning given to the words *šar* and *qav*; but some difficulty remains in the reference to a foreign tongue, i.e., the language of the Assyrians, in v. 11.

The author does not regard the cuneiform alphabet of Ras Shamra as the earliest attempt at a Semitic alphabet, but as 'an experimental attempt to adapt the cuneiform to the alphabetic system in the light of the Phoenician alphabet' (p. 104). He returns in the third lecture to the question of the relation between the signs of the Ras Shamra, or Ugaritic, alphabet, and the Accadian syllabary on the one side, and the North and South Semitic alphabets on the other. He rejects, and rightly, the view which would derive the Ugaritic signs wholly from either source, and states his own position as follows:

' The inventor of this alphabet was in any case no mere copyist but rather an experimenter who was not afraid of novelties which might not and in fact did not survive his system. He was acquainted with the use of writing-clay and the cuneiform script and chose it perhaps because it was more suitable for long records than stone and less perishable than papyrus ; but, being aware of the difficulties inherent in a syllabary, he preferred to devise an alphabet on the Egyptian or Phoenician model, of which he must have been aware, since recent excavation has shown that the Phoenician alphabet antedates the Ugaritic texts and Egyptian influence has been traced at Ugarit. Excavation, too, has shown that the period *c.* 1750-1000 B.C. was one in which experiments in writing were being made, and the obvious conclusion is that the Ugaritic method was one of these experiments. The inventor would be likely to borrow what seemed to him suitable or advantageous in the experiments made by neighbouring workers and add or adapt it to his own system ; for almost every invention is based on previous discoveries. His system, however, was invented too late to oust the Phoenician method which already held the field ; and indeed it scarcely lasted a generation, since clay was not so convenient and handy a medium of writing as papyrus, which therefore won the day. Date, medium and method, combined to ensure its defeat ' (p. 151 f). A point of some importance arises from the statement that the Ugaritic script scarcely lasted a generation ; the important collection of religious texts which have occupied the attention of scholars during the last twenty years contains the ancient myths and rituals of the Semitic population of this part of the Near East. If these traditions were only written down in the Ugaritic alphabetic script during the closing years of that civilization, we have to ask in what language and in what script did they exist before they received the form in which we have them ? From the archaeological evidence it would appear that the civilization of Ugarit was destroyed by the inroads of the Sea-peoples about the end of the 12th century B.C., after which date the alphabetic cuneiform of Ugarit ceased to be used. Among the tablets from Ugarit are some, probably of 14th century date, which present the curious phenomenon of Accadian written in alphabetic cuneiform. and one, published by Thureau-Dangin, R.A. xxxvii, 3, which is bilingual, Accadian and Ugaritic. Hence it is clear that considerably more than a generation before the disappearance of the Ugaritic civilization, some interchange was going on between the Accadian and Ugaritic languages and scripts. The Tell el-Amarna letters, which are contemporary with much of the literary material from Ugarit, show Canaanite glosses written syllabically in cuneiform, and this fact may furnish a possible answer to the question of the form in which the Ugaritic traditions and myths existed before the invention of that remarkable cuneiform alphabetic script which seems peculiar to Ugarit.

Professor Driver's book is of such an encyclopaedic character that it is impossible to discuss the many points of interest which it evokes, but mention must be made of the very interesting account and sound criticism of the various theories relating to the names and order of the letters of the Semitic alphabets. We shall close our review of this learned, valuable, and most interesting book by quoting Professor Driver's admirable summary of the conclusions to which his study of the problems of the origins of the alphabet has brought him : ' The conclusion of the matter then is this. The Sumerians invented writing on clay by means of pictographic signs and devised a method of using these to render syllables ; they also accidentally isolated four of the five vowels. The Babylonians developed the use of these signs for syllables and employed this syllabic script in continuous texts of every kind, interspersed with ideographs ; the Persians invented the simplest form of syllabic script based on the cuneiform system. The Egyptians had early devised their own system of hieroglyphs which they carried forward

through the hieratic and demotic stages of cursive writing ; they also adapted their signs for occasional use as syllables and even as consonants but never used them so in continuous texts except for a brief experimental period. It was the merit of the western Semites that they saw the importance of this discovery and, discarding the whole cumbrous machinery of ideographic and syllabic scripts and providing that each sound was represented by only one sign, made a simple alphabet the vehicle of written thought. Who first took this step is and may always remain unknown ; all that can be said is that he or they were sprung in all probability from one or other of the Semitic peoples who came into contact with the Egyptians *c.* 2500-1500 B.C. and that it was taken in or near Egypt, and that the invention was developed in Palestine and perfected on the Phoenician coast. At this early stage three types of alphabetic script were evolved, a mixed pictographic-linear, a cuneiform, and a true linear script ; the two former soon died out while the latter survived to be carried by the Phoenicians overseas to Greece, whence it passed to all the nations of the western hemisphere—one, and only one, of the gifts of the Semites to mankind.'

S. H. HOOKE.

ART IN MEDIEVAL FRANCE, 987-1498. *By* JOAN EVANS. *pp.* xxviii+300, with 280 plates. *Oxford: Geoffrey Cumberledge, Oxford University Press, 1948. Price 63s.*

Dante as he met Virgil at the beginning of his pilgrimage expressed to his master the following hope :

Vagliami 'l lungo studio e'l grand'amore
Che m'hanno fatto cercar lo tuo volume.

Dr Joan Evans, we feel, must have approached medieval France in much the same spirit. Her knowledge of the subject appears on every page, but knowledge alone would not have produced this fascinating book. It is inspired with a deep love for her subject, the art of France in the Middle Ages. Her approach can be best explained by quoting from her preface. 'My whole endeavour . . . is to show that French medieval art took the forms it did because of the needs of the men who commissioned it . . . We falsify history if we regard any kind of art as a series of specimens ranged in museums, impersonal and without reasons or background . . . The medieval art of France has to be studied in castles where no one lives, monasteries where none takes his vows, and, often, in churches where no one prays. Only by an effort of the imagination can we endow them with their true significance, but the effort will make the stones live'.

The production of the book is excellent and the 280 plates provide a well chosen view of French medieval art. The text covers the whole of the field and is supplemented with many footnotes and references, which will enable the reader in search of fuller information to follow the vistas opened up by her treatment of the subject. This is so wide that there will inevitably be points of detail on which the specialist will differ. We have noted a few, but these do not affect the value of the work. In the wider field it would be difficult for anyone less conversant with the whole range of material adequately to challenge a thesis, which carries conviction.

The major part of the book is concerned with ecclesiastical art, starting with the great Benedictine revival of the 10th and 11th centuries. The evolution of the churches is discussed with particular reference to the great pilgrimage churches and their influence. The ornament of these buildings and the sources from which it was derived are fully considered. 'Usually', concludes the author, 'the painter's and sculptor's model was provided by the illuminated manuscripts in the abbey library'. This is only one

instance of the way in which one art is shewn to be dependent on another. The purist might cavil at the inclusion of Cluniac churches under the heading Benedictine, but there is sufficient justification on artistic grounds; less happy is the extension to cover a church of canons like St. Martin of Tours. Further chapters deal with other religious bodies, the Cistercians, the Bishops and Chapters, the Augustinian Canons, the Mendicant Orders and the Carthusians, shewing how the special requirements of each are reflected in their buildings and their ornaments. The influences apparent do not spring only from the ecclesiastics; in many cases the taste of the lay donors was responsible. This factor tended to increase towards the end of the period, which provides a good example in the Carthusian church of Champmol, where the Dukes of Burgundy were buried. Here much of the sculpture 'can fairly be considered courtly rather than monastic'.

The chapter dealing with the king and court also throws much light on the development of the secular art of France during the Middle Ages. We see the bare furnishings of the stark military castle giving way to a gradually increasing luxury, which eventually culminated in the brilliant flower of the courtly art of the early Valois. Watching this evolution we begin to realize how different the surviving buildings must have looked before the colour faded from the stonework, before they were stripped of paintings and hangings, before the rich plate and silks were removed and destroyed by the envy and cupidity of a later age. A good example of the method which Dr Evans employs to bring home her argument is the juxtaposition of a view of the great hall of the Palace of the Dukes of Berri at Poitiers and a reproduction of the miniature in the Très Riches Heures, shewing the Duke at table in his hall, surrounded by the courtiers and the retinue of his household. Such comparisons explain the apparent austerity of apartments like the Tour Maubergeon of the same Palace where the broad, flat spaces on the walls cry aloud for the rich tapestries which once adorned them. This fair civilization, courtly and aristocratic, was ruined by the Hundred Years War and the miseries which accompanied that protracted struggle. When, under Louis XI, France emerged again into more tranquil waters medieval art was already in decline. The flamboyant, which is characteristic of the last century of the Middle Ages, was, as the author recognizes, essentially sterile. The future was with the new Italian style of the Renaissance, on which she passes so harsh a judgment.

C. A. RALEGH RADFORD.

RIO GRANDE GLAZE PAINT WARE; a study illustrating the place of ceramic technological analysis in archaeological research, by ANNA O. SHEPARD. *Contributions to American Anthropology and History*, vol. VII, no. 39. *Carnegie Institute of Washington*. 1942.

From time to time experiments have been made in the comparative analysis of pottery by spectrograph or by petrographic microscope, and also by firing tests, notably in Mrs Ehrich's *Report on the Early pottery of the Jébeleh Region* and in a study on the Iron Age pottery of Tell Beit Mersim (*A.A.S.O.R.* XXI-XXII). The results have been rather inconclusive, mainly through lack of comparative material, but due also to the expense in obtaining expert examination and advice.

An expansion and simplification of the technique of petrographic analysis has recently been carried out in America on a large scale and one may at last see the results of comparison over a wide series of material. Miss Anna Shepard's study of the Rio Grande glaze paint ware, the result of some years work on the material in the Pecos area, is based on the classification of sherds by the temper, or degreassant. The findings are most interesting both from the technological point of view and from the archaeological distribution of types.

The pottery selected for the survey was well-known over an area some 320 km. long by 160 km. wide. The material examined was from both excavated sites giving a stratified series, and from a large number of sherds collected from sites surveyed in the area. This material had already been studied typologically and stratigraphically, and the technological study was made as a check, as to the region in which the pottery was made.

The system used for the technical classification was Temper identification in a large number of sherds to provide the statistic required. 'The preparation of thin sections of pottery for analysis in polarized light requires so much time that it is not practical to identify great numbers in this way. It has been demonstrated, however, . . . that once a tempering material has been accurately identified with the petrographic microscope and its peculiarities determined, it is possible within certain limits to recognize it by examining a fresh break of the pottery with a binocular microscope . . . Identifications can be rapidly made in this way, thus making it possible to examine the entire contents of a stratigraphic test. The reliability and usefulness of the method is largely dependent upon its joint use with the petrographic microscope, and both methods were employed throughout . . .'. Over 17,000 sherds were examined and the resultant data has been summarized on maps and graphs, giving very satisfactory additional information to the archaeological results, and in some cases, making substantial corrections. Further subdivision of the pottery types and distribution was made possible by the temper designation, and in other cases, the place of manufacture was identified through the geological source of the tempering material.

The study is long and detailed, with special attention to the methods of use, the author being well aware of its limitations. However the results deserve very careful consideration by archaeologists in all parts of the world. An important factor is the use of the binocular microscope by a technician with more elementary training. Under the expert guidance of the petrologist, a large quantity of material can be economically examined with some 90 per cent accuracy. Secondly, the information gained in the production and distribution of a particular ware was most significant even when at variance with previous archaeological analysis.

This paper deserves wide study in the application of technical research on ancient pottery and is indicative of the way in which expert technical advice and co-operation can be really helpful to the archaeologist. Were this method to be applied, say, to Mycenaean pottery, one might expect some far reaching and interesting results.

J. du P.T.

MEROWINGERZEIT. By GUSTAV BEHRENS. (Römisch-Germanisches Zentralmuseum zu Mainz, Katalog 13; Original Altertümer des Zentralmuseums zu Mainz). Mainz, 1947. pp. 83, figs. 165, pl. 8. No price indicated.

It is a pleasure to welcome one of the first products of sound German archaeological work since the War. This is a Catalogue of Merovingian Antiquities in the Mainz Central Museum. Practically every piece listed is illustrated, generally by a line drawing, sometimes by a half-tone block. Except in certain instances where a fuller account is demanded, descriptions are very brief, but in conjunction with the corresponding sketch, suffice to give a clear and accurate picture of the object.

Pages 1-35 cover antiquities from the cemetery at Schwarzhheindorf—eighty-seven grave groups and a quantity of loose finds. Some of the best pieces from the grave-groups were sold to museums in Berlin, Bonn, Frankfurt-am-Main and Hamburg before the bulk of the material reached Mainz. These pieces, though not in the Mainz Museum,

are also listed, so that the grave groups are given complete, in accordance with the excavator's record. The cemetery is rich in combs, beads, brooches and pins, but particularly in glass and pottery. The latter includes an unusual proportion of wide bowls, and much of it is of hard, wheel-turned light-coloured often reddish ware, having a distinctly Roman look. After the grave groups, unassociated finds from the same cemetery are dealt with by types. They include buckets, weapons, pottery and some interesting brooches, three in the form of equal-armed crosses. The description of the grave goods is preceded by some general comments on the cemetery and bibliographical references to it. After the description of the grave goods there is a list giving particulars of the pieces from the cemetery that went to other museums with their present whereabouts, an analysis of the furnishing of men's and women's graves, and some archaeological comments on the finds.

The next group of objects (pp. 35-9), from the Heerdt Collection acquired in 1932, have no provenance and are not in grave groups. The pieces themselves suggest a Middle Rhine origin. Several good bird-brooches, some pendant crosses and others for attachment, and an exact parallel (previously published in *Mainzer Zeitschrift*, 1933, Taf. 18, 3) to the openwork disc-pendant with 'griffins' from Andernach in the Ashmolean Museum (E. T. Leeds, *Early Anglo-Saxon Art and Archaeology*, Pl. xvii, c.) may be mentioned.

The antiquities from the Fliedner Collection, purchased in 1928 (pp. 39-68) are more numerous. Weapons of all types, buckles, strap-ends, square-headed, radiate and round brooches, the latter mostly decorated with garnets or stamped-foil designs; circular openwork ornaments in geometric pattern, combs, ornamented pins and other miscellanea are well represented. The sheet-bronze ornamental mountings of a wooden casket, showing stamped and repoussé ornament of concentric circles in rectangular fields, bordered with broad bands of punched dot patterns, is particularly interesting (this piece is already published by Alföldi, *Acta Archaeologica* v, 1934, 'Eine spätromische Helmform' S. 114, Taf. 9, 2). But the main interest of this collection lies in the wide range of ceramic types, including bottle-vases very similar to those from the Faversham cemetery, as well as handled and spouted jugs, stamped urns, and bowls; about a hundred and twenty pottery vessels being illustrated.

The Catalogue concludes with a section devoted to miscellaneous Merovingian antiquities, most from Germany (pp. 69-81). Notable amongst these are a small pail ornamented with bronze mounts in the style of that from Beauvais (Leeds, op. cit., fig. 4) and the well-known Buire sur l'Ancre bucket (Leeds, op. cit., fig. 5); and an unusual handled and lidded bronze ewer (Abb. 158), paralleled at Dettingen (Veeck, *die Alamannen in Württemberg*, S. 30, Taf. 20, B4). The quality of the eight concluding plates is naturally enough, in view of the acute shortage of materials in Germany at the present time, not of the best, but the publication as a whole is a useful picture-book and place of reference for Middle-Rhenish antiquities of the Merovingian period, which are so closely related in many ways with finds from Kent.

R. L. S. BRUCE-MITFORD.

THE NEOLITHIC PERIOD IN BULGARIA. (American School of Prehistoric Research. *Bulletin no. 16*). By J. H. GAUL. Cambridge, Mass: 1948. pp. xxv and 251. Plates I-LXIX.

The Bulletin contains an obituary of G. G. MacCurdy, the founder and latterly Director of the American School, and a long list of his publications; also an obituary of J. H. Gaul who spent more than a year on archaeological research in Bulgaria in 1938-9, was chased and captured by the Nazis in Slovakia and shot in 1945. By his death, at the age of 34, European Archaeology suffered a tragic loss.

REVIEWS

The main body of the Bulletin is a general descriptive account, largely by sites, of Bulgarian Prehistoric Archaeology. The illustrations are good (though they suffer from the lamentable defect of having no scale and giving no references to the text) and the publication will be useful to all students of this important but complicated subject, as a great deal of the material has not previously been available in a W. European language.

The author first describes the W. Bulgarian Painted Culture: this, he suggests, came from the Morava Region and also up the Struma some time at the end of the 4th millennium B.C. And this, in spite of the evidence from Karanovo (pp. 20 and 44) where remains of this culture are reported from a level above the typical Mound Culture.

The largest section naturally deals with the Bulgarian Mound Culture and its predecessor Boian-A. Although he differentiates these two, the differences are not always easy to follow, and Boian-A does not apparently occur in Bulgaria except at the lower levels of the Mound Culture Sites. Boian-A he dates about 3000 B.C. and the Mound Culture 2500-1900 B.C.

Later chapters deal with the Bronze Age, but much of the material figured is more likely to be Iron Age, e.g. Thumb-grip Handles (Plates XIX.3 and LXIX.2), cf. Chauchitsa (Casson, also Heurtley). In spite of a great mass of prehistoric material, there is very little that could be referred to a Bronze Age as that term is understood in neighbouring countries, and it seems to the reviewer that the Mound Culture lasted a very long time and possibly right down to what is normally known as the Iron Age.

The only solution for these problems is further scientific excavation, and considering the geographical position of Bulgaria it is of the greatest importance.

A number of misprints occur: e.g. *re* Plates p. 36 for XI read XII; p. 55 for XLIX, XLV; p. 136 for XXVII, 3 XXV.4 and for XXV,5, XXV,3; p. 147 for XLII.5, XLII.2: p. 198 for XLVI.7,8, read XLVI.5,6.

Might one suggest, with hesitation, that the pot figured on Plate XIII from K. Dermen is medieval? The suggested parallel from Tsangli is not convincing. D. A. J. BUXTON.

MYCENAE—an Archaeological History and Guide. By ALAN J. B. WACE. pp. XVIII, 150, 110 plates. Princeton University Press (London, Geoffrey Cumberlege). Price \$15 (England, 120s).

This magnificently produced book by the foremost living authority on Mycenae makes available the full, accurate, and detailed description of the site we have so long been in need of. The publishers state in their notice 'the volume will not only be used by students of history and archaeology, but will serve as an indispensable guide for the traveler (*sic*) at the site'. The first part of the statement is undoubtedly true, but though the traveller may, and should, study this book in the 'Inn of the Fair Helen of Menelaus', so rightly recommended by the author, before and after his visit, it may be doubted whether the size and weight of the volume will commend itself to him as a guide book to be carried in the hand when on the site itself.

This is the first full description of Mycenae in English since Frazer described it in 1898 in his edition of Pausanias, and a comparison of the two shows the great advance that has been made in knowledge both of the site of Mycenae itself and of the whole of the prehistory of Greece as a result of the excavations and research of the last 50 years. It is fitting that it should be Wace, who has summarized and described these advances, many of which are the results of excavations that he has himself carried out. His name is now as indelibly associated with Mycenae as is Schliemann's.

The book begins with an evocative chapter on the topography of Mycenae in which all the author's feeling for the Greek countryside is expressed in restrained but vivid

language, such that even those who have never visited Greece can feel the charm and fascination of that combination of modern scenery and past glory which is the country's special appeal.

After this description of the site there are three general chapters, on its chronology, its tomb types, and its history. In this last chapter a paragraph only is devoted to the history of the excavation. A more detailed story would have been useful here as it is only with a knowledge of the work of the various excavators from Schliemann on that the changing views on the chronology and significance of the site can be understood.

Chapter VI starts the detailed description which forms the main part of the book. Beginning with the Treasury of Atreus the reader is led round the other tombs outside the citadel and then up to the citadel itself. The descriptions are clear and admirable, but there is more than just straightforward description; all the time the author is making new contributions to our knowledge, as in his brilliant reconstruction of the façade of the Treasury of Atreus, and in the publication of the delightful little ivory group of two women and a child found in the Palace during the 1939 excavations.

The book is not merely a dry description of architectural features. An attempt is made all the time to explain and interpret the function of the various parts of the complex and to use them to throw light on the life of the time when they were built, and in the final, all too short, chapter a summary is given of the present state of knowledge of the material culture of Mycenae. What is lacking here is perhaps any attempt to reconstruct the social relationships of the people, admittedly a difficult task in the absence of written records, but one which with the richness and variety of the archaeological remains could well be attempted. Childe with far poorer archaeological material has made a case for his theories about the social relations of the early Scots; the scholars of Mycenaean archaeology could well attempt a similar treatment for their material.

The important problem of the date of the Treasury of Atreus is dealt with in an appendix which is substantially the same as the author's article in this journal (1940, p. 233 ff) where the case for the 14th century date as against an earlier one is carefully argued. It is a measure of Wace's contribution that this view regarded as heretical in 1926 now commands a great measure of support.

P. L. SHINNIE.

LES FOUILLES PRÉHISTORIQUES. By A. LEROI-GOURHAN. (*Technique et Méthodes*), with an appendix by A. LAMING. Paris: A. and J. Picard, 1950.

MANUAL OF ARCHAEOLOGICAL SURVEYING. By A. H. DETWEILER. *New Haven: American Schools of Oriental Research*, 1948.

It is eighteen years since I reviewed a French manual of archaeological excavation (ANTIQUITY VI, 55), and the present booklet by M. A. Leroi-Gourhan, director of the 'centre de documentation et de recherches préhistoriques du Musée de l'Homme', is a straw that shows which way the wind has blown in the meantime. Very little wind and not much of a straw: its effective length is only 30 small pages, to which appendices and diagrams are added. As is natural in France, it betrays a preoccupation with cave-hunting, but it touches lightly, very lightly, upon a number of reasonably modern generalities, with a recurrent note of apology that disarms criticism. 'Chez nous encore, de nombreux fouilleurs usent d'une méthode plus scientifique (than formerly), mais pourtant périmée depuis longtemps à l'étranger', '... l'idée, déjà bien vieillie à l'étranger ...', and the like, indicate an inferiority-complex which, in spite of obvious good intention, this booklet amply justifies. The appendix, dealing with special skills such as pollen-analysis, tree-ring analysis and even 'coal fourteen', is pathetically lacking in significant detail, and the bibliographies are remarkable alike for their

omissions and their inclusions. 'Air-photography', for example, confines its book-list to a popular French picture-book and Baradez's (excellent) recent *Fossatum Africae*, with no mention of the pioneer-work of Crawford and others and no proper reference even to Poidebard. The cultural background of the book is indeed charmingly implied in the following paragraph: 'Le moment le plus dangereux est celui d'une vraie grande découverte; l'exaltation risque de devenir telle que le fouilleur oublie la moitié des consignes, pousse des exclamations admiratives et s'aperçoit trop tard que sa documentation est incomplète. Il est vraiment grand s'il a le courage de s'asseoir et d'allumer une cigarette pour réfléchir'—preferably, we may add, with the aid also of R. J. C. Atkinson's *Field Archaeology*, which need certainly have no fear of its new Gallic rival. Incidentally, throughout the book the *préhistorien* is scrupulously distinguished from the *archéologue*, a subtlety which escapes the understanding of the foreign reader, though it is evident that the *archéologue* is a cruder species, inclined to 'travail brutal'.

Detweiler's book is more limited in scope, though not so limited as its title implies. The author is professor of architecture at Cornell University and has participated in excavations in Palestine, Syria and Mesopotamia. His advice is therefore of an experienced and practical kind, and in so far as it is limited to actual survey it fills a need. But it is evident that, on the sites where he has operated, his operations were by no means so restricted. He includes in the surveyor's task responsibilities for which, *qua* surveyor, he has no qualification. The surveyor, he affirms, must be ready himself to 'train the workmen in the technique of exposing the building walls with a minimum of damage'. 'He is also responsible for establishing the loci of all non-architectural objects that are found'. Nevertheless, 'he will always have a great deal of surveying and drafting to do, so he must limit the time he spends supervising actual excavation. *If, however, other members of the staff can be trained to recognize and protect architectural remains as they appear, the architect can budget his time so that only occasional visits to the actual digging area are necessary*' (my italics). What a vision of muddle all this (and more like it) conjures up! The background is that of a typical pre-war Near Eastern excavation where stratification was ill-understood, assistants ill-trained, and supervision sketchy. Detached from this unhappy background and restricted to the proper implication of its title, the book would be a useful guide to the student in a matter where guidance is peculiarly necessary.

R.E.M.W.

GLASS AND GLAZES FROM ANCIENT EGYPT. By MRS ELIZABETH RIEFSTAHL.
Brooklyn Museum, 1948. 24 pp., 36 figs. 40 cents.

This is an excellent little handbook and can unhesitatingly be recommended as an elementary guide to the subject, even for those who may never see the fine Brooklyn collections.

Museum guides are not usually, nor need they be, the product of deep research, and this one is no exception; but I know of no other place where the basic information about Egyptian glass and glazes is set out so lucidly and so effectively in non-technical language. Unfortunately, however, the illustrations, which are at times fuzzy and apparently out of focus, are not up to the standard of the text; and it is a pity that more pictures of glass vessels were not available for inclusion, for the four small New Kingdom vessels in fig. 1—the only ones illustrated—are far from representative of dynastic Egyptian glassware as a whole.

There are a few points which should be looked into when the time comes, as it must, for a second edition. It is doubtful whether there is any truly black glass known from antiquity (p. 5). There is much glass which appears to be black, but when seen in very

thin section, it proves to be really a brown metal. The grotesque face-pendants (fig. 17) are surely 5th-4th century B.C. at latest, rather than Hellenistic or Roman. On the other hand the large Aphrodite figurine (fig. 22) is not likely to be earlier than Roman, for the type resembles closely many small Roman bronze Aphrodites. The spotted lion (fig. 8) which Mrs Riefstahl will not date, would seem to be a typical Middle Kingdom product. And, finally, Lucas's article on Glazed Ware in Egypt (*J.E.A.*, XXII) should be added to the otherwise excellently eclectic bibliography.

D. B. HARDEN.

BEYOND THE BOUNDS OF HISTORY : Scenes from the Old Stone Age, by HENRI BREUIL. English Translation by MARY E. BOYLE : Foreword by FIELD-MARSHAL J. C. SMUTS. *P. R. Gawthorn Ltd., 55 Russell Square, London, W.C. 1*, 1949. 12s 6d.

This is a book of pictures of prehistoric life as it was lived by our remote ancestors for thousands of years ; it was the normal life of all mankind until quite recently, and if the child is father of the man, we must all inherit much from that childhood of our species. Many attempts have been made to reconstruct such early life pictorially, but most of them have been by persons who had little or no first-hand acquaintance with the facts. The value of these drawings lies in the fact that they are by the Abbé Breuil, who is universally acknowledged as the greatest living authority on Old Stone Age man and his works, and on the other animals amongst whom he lived. No one has ever had such a range of first-hand contact with the facts of prehistory in three continents ; and it is that which gives a unique value to his imaginative drawings, counterbalancing a certain crudeness of execution that may even have merits of its own. The author has two other qualifications—a knowledge of animals and a sense of humour. No one can sympathetically understand prehistoric life unless he has, like the author, a countryman's feeling for the animals that played so large a part in it. That feeling is not necessarily friendly, and prehistoric man's feelings were probably mixed ; it might best be described perhaps as a sense of community. (One suspects, for instance, that the modern farmer prefers his cattle to some of his urban customers).

The book is partly intended for young people—who have made several important discoveries of prehistoric man's handiwork. It is well produced ; the many scenes are reproduced in colour, and we confidently recommend the book to schools and all who want to understand what life in those ' good old times ' was really like.

O.G.S.C.

THE LUNGFISH AND THE UNICORN : an excursion into romantic zoology. By WILLY LEY. *Hutchinson's Scientific and Technical Publications*, 1948. 254 pages. Price 10s 6d.

Do not be put off by the dust-cover or title of this book. It is both trustworthy and readable, and it is briefly reviewed here only because it touches no more than the fringes of archaeology. It is permeated with the idea of evolution, and the author is obviously familiar also with the history of zoology—which is one reason why it is so readable. Another is the author's style which, in spite of occasional lapses, is simple and direct. Unicorns are fair game for archaeologists, and it does seem that Mr Ley has run the myth to earth—in a mistranslation in the Bible. We particularly relish the story (of Jewish origin) that it was too big to go in the ark and had to spend the time swimming about round the Ark occasionally resting the tip of its horn thereon—swimming on its back no doubt, as we do when resting in the water.

Chapter XIV, the story of Gondwanaland, would have been improved by a sketch-map showing the extent of that lost continent (or a series of such, showing its gradual break-up) : which leads to the suggestion that Mr Ley should write another book dealing on zoological lines with palaeogeography—marginal survival and that sort of thing, not omitting man himself.

O.G.S.C.